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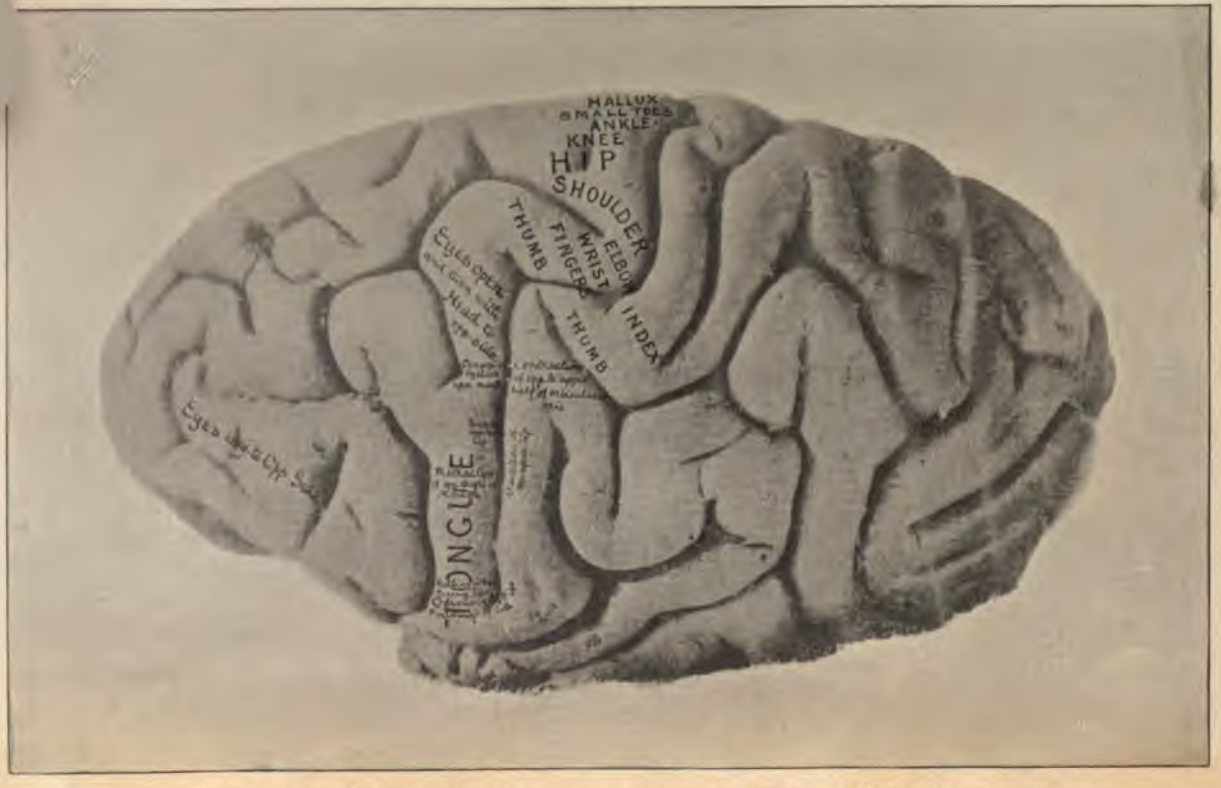
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EXPERIMENTS ON ANIMALS





Brain of an Anthropoid Ape, showing the position of the Motor Centres.

EXPERIMENTS ON ANIMALS

BY

STEPHEN PAGET

WITH AN INTRODUCTION BY

LORD LISTER

THIRD AND REVISED EDITION

"Perhaps it is wrong to compare sin with sin, but I declare to you, the more I think of it, the more intimately does this Prejudice seem to me to corrupt the soul, even beyond those sins which are commonly called more deadly."—CARDINAL NEWMAN.

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TO

CHARLES ALFRED BALLANCE

M.S., F.R.C.S.

AND

WILLIAM HUNTER

M.D., F.R.C.P.



PREFACE

THE first edition of this book was published in 1900. For twelve years it had been my business, as Secretary to the Association for the Advancement of Medicine by Research, to know something about experiments on animals, and to follow the working of the Act of 1876; and to give facts and references to a very large number of applicants. Believing that an account of these experiments, and of the conditions imposed on them by the Act, might serve a useful purpose, I proposed to the Council of the Association that I should write a book on the subject. The Council accepted this proposal; and decided that the book should be written for general reading, that it should not be anonymous, and that it should be published without reserve.

It was, of course, a doubtful and embarrassing task. But, from twelve years' experience of the things said by the chief opponents of all experiments on animals, I knew that there was only one way of doing it—to give the original authorities, the plain facts, the very words, chapter and verse for everything.

Among those who kindly revised the proofs were Prof. Rose Bradford and Prof. Starling, who revised Part I.; Mr. Shattock, who revised Part II.; and Prof. Schäfer. Valuable help was given by Mr. R. H. Clarke, Sir Victor Horsley, Dr. Beevor, Prof. Ronald Ross, and the late Dr. Washbourn; and I was allowed to

make free use of Mr. George Pernet's careful researches into the history of the subject. Lord Lister himself did me the honour to read and correct, with the utmost patience, Parts I. and II.

In the second edition (1904) some mistakes were corrected, and some facts were added.

The present edition has been thoroughly revised; and I have included in it a reprint, with some changes and omissions, of a pamphlet, *The Case against Anti-vivisection*, which I wrote in 1904.

1906.

INTRODUCTION TO THE FIRST EDITION

This work by Mr. Paget is entirely a labour of love. Not being himself engaged in researches involving experiments upon the lower animals, he is not directly interested in the subject. But, in his official capacity as Secretary (1887–1899) to the Association for the Advancement of Medicine by Research, he has become widely conversant with such investigations, and has been deeply impressed with the greatness of the benefits which they have conferred upon mankind, and the grievous mistake that is made by those who desire to suppress them.

The action of these well-meaning persons is based upon ignorance. They allow that man is permitted to inflict pain upon the lower animals when some substantial advantage is to be gained; but they deny that any good has ever resulted from the researches which they condemn.

How far such statements are from the truth will be evident to those who peruse this book. Its earlier pages deal with Physiology, the main basis of all sound medicine and surgery. The examples given in this department are not numerous; they are, however, sufficiently striking, as indications that, from the discovery of the circulation of the blood onwards, our knowledge of healthy animal function has been mainly derived from experiments on animals.

The chief bulk of the work is devoted to the class of investigations which are most frequent at the present day; and it shows what a flood of light has been already thrown by Bacteriology upon the nature of human disease and the means of combating it.

The chapter on the Action of Drugs will be to many a startling disclosure of the gross ignorance that prevailed among physicians even in the earlier part of last century. The great revolution that has since taken place is no doubt largely due to advances in sciences other than Biology, especially Chemistry. But it could not have attained its present proportions without the ever-increasing knowledge of Physiology, based on experiments on animals; and Mr. Paget shows how large a share these have had in the direct investigation of articles of the Materia Medica.

The concluding part of the volume discusses the restrictions which have been placed by the legislature in this country on those engaged in these researches, with the view of obviating possible abuse. Whether the Act in question has been really useful, whether it has not done more harm than good, by hampering and sometimes entirely preventing legitimate and beneficent investigation, I will not now discuss.

Meanwhile I commend Mr. Paget's book to the careful consideration of the reader.

LISTER.

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PART I

EXPERIMENTS IN PHYSIOLOGY



EXPERIMENTS ON ANIMALS

I

THE BLOOD

1.—Before Harvey

Galen, born at Pergamos, 131 A.D., proved by experiments on animals that the brain is as warm as the heart, against the Aristotelian doctrine that the office of the brain is to keep the heart cool. He also proved that the arteries during life contain blood, not πνεῦμα, or the breath of life :—

"Ourselves, having tied the exposed arteries above and below, opened them between the ligatures, and showed that they were indeed full of blood."

Though all vessels bleed when they are wounded, yet this experiment was necessary to refute the fanciful teaching of Erasistratus and his followers, of whom Galen says :—

"Erasistratus is pleased to believe that an artery is a vessel containing the breath of life, and a vein is a vessel containing blood; and that the vessels, dividing again and again, come at last to be so small that they can close their ultimate pores, and keep the blood controlled within them; yea, though the pores of the vein and of the artery

lie side by side, yet the blood remains within its proper bounds, nowhere passing into the vessels of the breath of life. But when the blood is driven with violence from the veins into the arteries, forthwith there is disease; and the blood is poured the wrong way into the arteries, and there withstands and dashes itself against the breath of life coming from the heart, and turns the course of it—and this forsooth is fever."

For many centuries after Galen, men were content to worship his name and his doctrines, and forsook his method. They did not follow the way of experiment, and invented theories that were no help either in science or in practice. Here, in Galen's observation of living arteries, was a great opportunity for physiology; but the example that he set to those who came after him was forgotten by them, and, from the time of Galen to the time of the Renaissance, physiology remained almost where he had left it. Of the men of the Renaissance, Servetus, Cæsalpinus, Ruinius, and others, Harvey's near predecessors, this much only need be said here, that they did not discover the circulation of the blood; and that the claim made a few years ago to this discovery, on behalf of Cæsalpinus, by his countrymen, was not successful. But it is probable that Realdus (1516–1557) did understand the passage of blood through the lungs, but not the general circulation. He says:—

"The blood is carried through the pulmonary artery to the lung, and there is attenuated; thence, mixed with air, it is carried through the pulmonary vein to the left ventricle of the heart: which thing no man hitherto has noted or left on record, though it is most worthy of the observation of all men. . . . And this is as true as truth itself; for if you will look, not only in the dead body but also in the living animal, you will always find this pul-

monary vein full of blood, which assuredly it would not be if it were designed only for air and vapours. . . . Verily, I pray you, O candid reader, studious of authority, but more studious of truth, to make experiment on animals. You will find the pulmonary vein full of blood, not air or *fuligo*, as these men call it, God help them. Only there is no pulsation in the vein." (*De Re Anatomicâ*, Venice, 1559.)

Fabricius ab Aquapendente, Harvey's master at Padua, published his work on the valves of the veins—*De Venarum Ostiolis*—in 1603. He did not discover them. Sylvius speaks of them in his *Isagoge* (Venice, 1555), and they were known to Amatus (1552), and even to Theodoretus, Bishop of Syria, who lived, as John Hunter said of Sennertus, "the Lord knows how long ago." But Fabricius studied them most carefully; and in anatomy he left nothing more to be said about them. In physiology, his work was of little value; for he held that they were designed "to retard the blood in some measure, lest it should run pell-mell into the feet, hands, and fingers, there to be impacted": they were to prevent distension of the veins, and to ensure the due nourishment of all parts of the body. It is true that he compared them to the locks or weirs of a river, but he understood neither the course nor the force of the blood: as Harvey said of him, "The man who discovered these valves did not understand their right use; neither did they who came after him"—*Harum valvularum usum rectum inventor non est assecutus, nec alii addiderunt; non est enim ne pondere deorsum sanguis in inferiora totus ruat; sunt namque in jugularibus deorsum spectantes, et sanguinem sursum ferri prohibentes.* Men had no idea of the rapidity and volume of the circulation; they thought of a sort of Stygian tide, oozing this way

or that way in the vessels—Cæsalpinus was of opinion that it went one way in the daytime and another at night—nor did they see that the pulmonary circulation and the general circulation are one system, the same blood covering the whole course. The work that they did in anatomy was magnificent; Vesalius, and the other great anatomists of his time, are unsurpassed. But physiology had been hindered for ages by fantastic imaginings, and the facts of the circulation of the blood were almost as far from their interpretation in the sixteenth century as they had been in the time of Galen.

II.—Harvey (1578–1657)

The *De Motu Cordis et Sanguinis in Animalibus* was published at Frankfurt in 1628. And it begins with these words: *Cum multis vivorum dissectionibus, uti ad manum dabantur:*—

"When by many dissections of living animals, as they came to hand, I first gave myself to observing how I might discover with my own eyes, and not from books and the writings of other men, the use and purpose of the movement of the heart in animals, forthwith I found the matter hard indeed, and full of difficulty: so that I began to think, with Frascatorius, that the movement of the heart was known to God alone. For I could not distinguish aright either the nature of its systole and diastole, or when or where dilatation and contraction took place; and this because of the swiftness of the movement, which in many animals in the twinkling of an eye, like a flash of lightning, revealed itself to sight and then was gone; so that I came to believe that I saw systole and diastole now this way now the other, and movements now apart and now together. Wherefore my mind wavered; I had nothing assured to me, whether decided by me or taken from other men: and I did not wonder

that Andreas Laurentius had written that the movement of the heart was what the ebb and flow of the Euripus had been to Aristotle.

"At last, having daily used greater disquisition and diligence, by frequent examination of many and various living animals—*multa frequenter et varia animalia viva introspiciendo*—and many observations put together, I came to believe that I had succeeded, and had escaped and got out of this labyrinth, and therewith had discovered what I desired, the movement and use of the heart and the arteries. And from that time, not only to my friends, but also in public in my anatomical lectures, after the manner of the Academy, I did not fear to set forth my opinion in this matter."

It is plain, from Harvey's own words, that he gives to experiments on animals a foremost place among his methods of work. Take only the headings of his first four chapters :—

i. *Causæ, quibus ad scribendum auctor permotus fuerit.*
ii. *Ex vivorum dissectione, qualis fit cordis motus.*
iii. *Arteriarum motus qualis, ex vivorum dissectione.*
iv. *Motus cordis et auricularum qualis, ex vivorum dissectione.*

He thrusts it on us, he puts it in the foreground. Read the end of his Preface :—

"Therefore, from these and many more things of the kind, it is plain (since what has been said by men before me, of the movement and use of the heart and arteries, appears inconsistent or obscure or impossible when one carefully considers it) that we shall do well to look deeper into the matter; to observe the movements of the arteries and the heart, not only in man, but in all animals that have hearts; and by frequent dissection of living animals, and much use of our own eyes, to discern and investigate the truth—*vivorum dissectione frequenti, multâque autopsiâ, veritatem discernere et investigare.*"

Finally, take the famous passage in the eighth chapter, *De copiâ sanguinis transeuntis per cor e venis in arterias, et de circulari motu sanguinis*:—

"And now, as for the great quantity and forward movement of this blood on its way, when I shall have said what things remain to be said—though they are well worth considering, yet they are so new and strange that I not only fear harm from the envy of certain men, but am afraid lest I make all men my enemies; so does custom, or a doctrine once imbibed and fixed down by deep roots, like second nature, hold good among all men, and reverence for antiquity constrains them. Be that as it may, the die is cast now: my hope is in the love of truth, and the candour of learned minds. I bethought me how great was the quantity of this blood. Both from the dissection of living animals for the sake of experiment, with opening of the arteries, with observations manifold; and from the symmetry of the size of the ventricles, and of the vessels entering and leaving the heart—because Nature, doing nothing in vain, cannot in vain have given such size to these vessels above the rest—and from the harmonious and happy device of the valves and fibres, and all other fabric of the heart; and from many other things—when I had again and again carefully considered it all, and had turned it over in my mind many times—I mean the great quantity of the blood passing through, and the swiftness of its passage—and I did not see how the juices of the food in the stomach could help the veins from being emptied and drained dry, and the arteries contrariwise from being ruptured by the excessive flow of blood into them, unless blood were always getting round from the arteries into the veins, and so back to the right ventricle—I began to think to myself whether the blood had a certain movement, as in a circle—*cœpi egomet mecum cogitare, an motionem quandam quasi in circulo haberet*—which afterward I found was true."

This vehement passage, which goes with a rush like that of the blood itself, is a good example of the width and depth of Harvey's work—how he used all methods that were open to him. He lived to fourscore years; "an old man," he says, "far advanced in years, and occupied with other cares": and, near the end of his life, he told the Hon. Robert Boyle that the arrangement of the valves of the veins had given him his first idea of the circulation of the blood:—

"I remember that when I asked our famous Harvey, in the only discourse I had with him, which was but a while before he died, what were the things which induced him to think of the circulation of the blood, he answered me that when he took notice that the valves in the veins of so many parts of the body were so placed that they gave free passage of the blood towards the heart, but opposed the passage of the venal blood the contrary way, he was invited to imagine that so provident a cause as Nature had not so placed so many valves without design; and no design seemed more probable than that, since the blood could not well, because of the interposing valves, be sent by the veins to the limbs, it should be sent by the arteries, and return through the veins, whose valves did not oppose its course that way."

But between this observation, which "invited him to imagine" a theory, and his final proofs of the circulation, lay a host of difficulties; and it is certain, from his own account of his work, that experiments on animals were of the utmost help to him in leading him "out of the labyrinth."

III.—After Harvey

1. *The Capillaries*

The capillary vessels were not known in Harvey's time: the *capillamenta* of Cæsalpinus were not the

capillaries, but the *νεῦρα* of Aristotle. It was believed that the blood, between the smallest arteries and the smallest veins, made its way through "blind porosities" in the tissues, as water percolates through earth or through a sponge. The first account of the capillaries is in two letters (*De Pulmonibus*, 1661) from Malpighi, professor of medicine at Bologna, to Borelli, professor of mathematics at Pisa. In his first letter, Malpighi writes that he has tried in vain, by injecting the dead body, to discover how the blood passes from the arteries into the veins:—

"This enigma hitherto distracts my mind, though for its solution I have made many and many attempts, all in vain, with air and various coloured fluids. Having injected ink with a syringe into the pulmonary artery, I have again and again seen it escape (become extravasated into the tissues) at several points. The same thing happens with an injection of mercury. These experiments do not give us the natural pathway of the blood."

But, in his second letter, he describes how he has examined, with a microscope of two lenses, the lung and the mesentery of a frog, and has seen the capillaries, and the blood in them:—

"Such is the divarication of these little vessels, coming off from the vein and the artery, that the order in which the vessel ramifies is no longer preserved, but it looks like a network woven from the offshoots of both vessels."

He was able, in a dead frog, to see the capillaries; and then, in a living frog, to see the blood moving in them. But, in spite of this work, it took nearly half a century before Harvey's teaching was believed by all men—*Tantum consuetudo apud omnes valet.*

2. *The Blood-pressure*

Harvey had seen the facts of blood-pressure—*the great quantity of blood passing through, and the swiftness of its passage*—but he had not measured it. Keill's experiments on the blood-pressure (1718) were inexact, and of no value; and the first exact measurements were made by Stephen Hales, who was rector of Farringdon, Hampshire, and minister of Teddington, Middlesex; a Doctor of Divinity, and a Fellow of the Royal Society. His experiments, in their width and diversity, were not surpassed even by those of John Hunter, and were extended far over physiology, vegetable physiology, organic and inorganic chemistry, and physics; they ranged from the invention of a sea-gauge to the study of solvents for the stone, and he seems to have experimented on every force in Nature. The titles of his two volumes of *Statical Essays* (1726-1733) show the great extent of his non-clerical work :—

Volume I. *Statical Essays, containing Vegetable Statics, or an Account of some Statical Experiments on the Sap in Vegetables, being an Essay towards a Natural History of Vegetation; also, a Specimen of an Attempt to Analyse the Air, by a great Variety of Chymio-Statical Experiments.*

Volume II. *Statical Essays, containing Hæmostatics, or an Account of some Hydraulic and Hydrostatical Experiments made on the Blood and Blood-vessels of Animals; also, an Account of some Experiments on Stones in the Kidneys and Bladder, with an Enquiry into the Nature of those anomalous Concretions.*

"We can never want matter for new experiments," he says in his preface. "We are as yet got little further than to the surface of things: we must be content, in

this our infant state of knowledge, while we know in part only, to imitate children, who, for want of better skill and abilities, and of more proper materials, amuse themselves with slight buildings. The farther advances we make in the knowledge of Nature, the more probable and the nearer to truth will our conjectures approach: so that succeeding generations, who shall have the benefit and advantage both of their own observations and those of preceding generations, may then make considerable advances, when *many shall run to and fro, and knowledge shall be increased.*"

His account of his plan of measuring the blood-pressure, and of one of many experiments that he made on it, is as follows:—

"Finding but little satisfaction in what had been attempted on this subject by Borellus and others, I endeavoured, about twenty-five years since, by proper experiments, to find what was the real force of the blood in the crural arteries of dogs, and about six years afterwards I repeated the like experiments on two horses, and a fallow doe; but did not then pursue the matter any further, being discouraged by the disagreeableness of anatomical dissections. But having of late years found by experience the advantage of making use of the statical way of investigation, not only in our researches into the nature of vegetables, but also in the chymical analysis of the air, I was induced to hope for some success, if the same method of enquiry were applied to animal bodies. . . .

"Having laid open the left crural artery (of a mare), I inserted into it a brass pipe whose bore was $\frac{1}{6}$ of an inch in diameter; and to that, by means of another brass pipe which was fitly adapted to it, I fixed a glass tube of nearly the same diameter, which was 9 feet in length; then, untying the ligature on the artery, the blood rose in the tube 8 feet 3 inches perpendicular above the level of the left ventricle of the heart, but it did not attain

to its full height at once: it rushed up gradually at each pulse 12, 8, 6, 4, 2, and sometimes 1 inch. When it was at its full height, it would rise and fall at and after each pulse 2, 3, or 4 inches, and sometimes it would fall 12 or 14 inches, and have there for a time the same vibrations up and down, at and after each pulse, as it had when it was at its full height, to which it would rise again, after forty or fifty pulses."

3. *The Collateral Circulation*

After Hales, came John Hunter, who was five years old when the *Statical Essays* were published. His experiments on the blood were mostly concerned with its properties, not with its course; but one great experiment must be noted here that puts him in line with Harvey, Malpighi, and Hales. He got from it his knowledge of the collateral circulation; he learned how the obstruction of an artery is followed by enlargement of the vessels in its neighbourhood, so that the parts beyond the obstruction do not suffer from want of blood: and the facts of collateral circulation were fresh in his mind when, a few months later, he conceived and performed his operation for aneurysm (December 1785). The "old operation" gave him no help here; and "Anel's operation" was but a single instance, and no sure guide for Hunter, because Anel's patient had a different sort of aneurysm. Hunter knew that the collateral circulation could be trusted to nourish the limb, if the femoral artery were ligatured in "Hunter's canal" for the cure of popliteal aneurysm; and he got this knowledge from the experiment that he had made on one of the deer in Richmond Park, to see the influence of ligature of the carotid artery on the growth of the antler. The following account of this

experiment was given by Sir Richard Owen, who had it from Mr. Clift, Hunter's devoted pupil and friend :—

"In the month of July, when the bucks' antlers were half-grown, he caused one of them to be caught and thrown; and, knowing the arterial supply to the hot 'velvet,' as the keepers call it, Hunter cut down upon and tied the external carotid; upon which, laying his hand upon the antler, he found that the pulsations of the arterial channels stopped, and the surface soon grew cold. The buck was released, and Hunter speculated on the result—whether the antler, arrested at mid-growth, would be shed like the full-grown one, or be longer retained. A week or so afterward he drove down again to the park, and caused the buck to be caught and thrown. The wound was healed about the ligature; but on laying his hand on the antler, he found to his surprise that the warmth had returned, and the channels of supply to the velvety formative covering were again pulsating. His first impression was that his operation had been defective. To test this, he had the buck killed and sent to Leicester Square. The arterial system was injected. Hunter found that the external carotid had been duly tied. But certain small branches, coming off on the proximal or heart's side of the ligature, had enlarged; and, tracing-on these, he found that they had anastomosed with other small branches from the distal continuation of the carotid, and these new channels had restored the supply to the growing antler. . . . Here was a consequence of his experiment he had not at all foreseen or expected. A new property of the living arteries was unfolded to him."

All the anatomists had overlooked this physiological change in the living body, brought about by disease. And the surgeons, since anatomy could not help them, had been driven by the mortality of the "old operation" to the practice of amputation.

4. *The Mercurial Manometer*

Hale's experiments on the blood-pressure were admirable in their time; but neither he nor his successors could take into account all the physiological and mathematical facts of the case. But a great advance was made in 1828, when Poiseuille published his thesis, *Sur la Force du Cœur Aortique*, with a description of the mercurial manometer. Poiseuille had begun with the received idea that the blood-pressure in the arteries would vary according to the distance from the heart, but he found by experiment that this doctrine was wrong:—

"At my first experiments, wishing to make sure whether the opinions, given *à priori*, were true, I observed to my great astonishment that two tubes, applied at the same time to two arteries at different distances from the heart, gave columns of exactly the same height, and not, as I had expected, of different heights. This made the work very much simpler, because, to whatever artery I applied the instrument, I obtained the same results that I should have got by placing it on the ascending aorta itself."

He found also, by experiments, that the coagulation of the blood in the tube could be prevented by filling one part of the tube with a saturated solution of sodium carbonate. The tube, thus prepared, was connected with the artery by a fine cannula, exactly fitting the artery. With this instrument, Poiseuille was able to obtain results far more accurate than those of Hales, and to observe the diverse influences of the respiratory movements on the blood-pressure. He sums up his results in these words:—

"I come to this irrevocable conclusion, that the force with which a molecule of blood moves, whether in the

carotid, or in the aorta, etc., is exactly equal to the force which moves a molecule in the smallest arterial branch; or, in other words, that a molecule of blood moves with the same force over the whole course of the arterial system—which, *à priori*, with all the physiologists, I was far from thinking."

And he adds, in a footnote :—

"When I say that this force is the same over the whole course of the arterial system, I do not mean to deny that it must needs be modified at certain points of this system, which present a special arrangement, such as the anastomosing arches of the mesentery, the arterial circle of Willis, etc."

Later, in 1835, he published a very valuable memoir on the movement of the blood in the capillaries under different conditions of heat, cold, and atmospheric pressure.

5. *The Registration of the Blood-pressure*

Poiseuille's work, in its turn, was left behind as physiology went forward: especially, the discovery of the vaso-motor nerves compelled physiologists to reconsider the whole subject of the blood-pressure. If Poiseuille's thesis (1828) be compared with Marey's book (1863), *Physiologie Médicale de la Circulation du Sang*, it will be evident at once how much wider and deeper the problem had become. Poiseuille's thesis is chiefly concerned with mathematics and hydrostatics; it suggests no method of immediate permanent registration of the pulse, and is of no great value to practical medicine: Marey's book, by its very title, shows what a long advance had been made between 1828 and 1863—*Physiologie Médicale de la Circulation du Sang, basée sur l'étude graphique des mouvements du cœur et du*

pouls artériel, avec application aux maladies de l'appareil circulatoire. Though the contrast is great between Hales' may-pole and Poiseuille's manometer, there is even a greater contrast between Poiseuille's mathematical calculations and Marey's practical use of the sphygmograph for the study of the blood-pressure in health and disease. Marey had the happiness of seeing medicine, physiology, and physics, all three of them working to one end:—

"La circulation du sang est un des sujets pour lesquels la médecine a le plus besoin de s'éclairer de la physiologie, et où celle-ci à son tour tire le plus de lumière des sciences physiques. Ces dernières années sont marquées par deux grands progrès qui ouvrent aux recherches à venir des horizons nouveaux: en Allemagne, l'introduction des procédés graphiques dans l'étude du mouvement du sang; en France, la démonstration de l'influence du système nerveux sur la circulation périphérique. Cette dernière découverte, que nous devons à M. Cl. Bernard, et qui depuis dix ans a donné tant d'impulsion à la science, montre mieux que toute autre combien la physiologie est indispensable à la médecine, tandis que les travaux allemands ont bien fait ressortir l'importance des connaissances physiques dans les études médicales."

Marey's sphygmograph was not the first instrument of its kind. There had been, before it, Hérisson's sphygmometer, Ludwig's kymographion, and the sphygmographs of Volckmann, King, and Vierordt. But, if one compares a Vierordt tracing with a Marey tracing, it will be plain that Marey's results were far advanced beyond the useless "oscillations isochrones" recorded by Vierordt's instrument.

Beside this improved sphygmograph, Chauveau and Marey also invented the cardiograph, for the observation of the blood-pressure within the cavities of the

heart. Their cardiograph was a set of very delicate elastic tambours, resting on the heart, or passed through fine tubes into the cavities of the heart,[1] and communicating impulses to levers with writing-points. These writing-points, touching a revolving cylinder, recorded the variations of the endocardial pressure, and the duration of the auricular and ventricular contractions.

It is impossible here to describe the subsequent study of those more abstruse problems that the older physiologists had not so much as thought of: the minutest variations of the blood-pressure, the multiple influences of the nervous system on the heart and blood-vessels, the relations between blood-pressure and secretion, the automatism of the heart-beat, the influence of gravitation, and other finer and more complex issues of physiology. But, even if one stops at Marey's book, now more than forty years old, there is an abundant record of good work, from the discovery of the circulation to the invention of the sphygmograph.

[1] "On peut s'assurer de l'innocuité de ce premier temps de l'expérience en examinant l'animal, qui n'est nullement troublé, qui marche et mange comme de coutume. En comptant le chiffre du pouls, on trouve quelquefois une légère accéleration, surtout dans les premiers instants ; mais les mouvements du cœur sont toujours réguliers, et donnent, à l'auscultation, des bruits d'un caractère normal." (Marey, *loc. cit.* p. 63.)

II

THE LACTEALS

Asellius, in his account of his discovery of the lacteal vessels (1622), is of opinion that certain of "the ancients" had seen these vessels, but had not recognised them. He has a great reverence for authority: Hippocrates, Plato, Aristotle, the Stoics, Herophilus, Galen, Pollux, Rhases, and a host of other names, he quotes them all, and all with profound respect; and comes to this conclusion: "It did not escape the ancients, that certain vessels must needs be concerned with containing and carrying the chyle, and certain other vessels with the blood: but the true and very vessels of the chyle, that is, my 'veins,' though they were seen by some of the ancients, yet they were recognised by none of them." He can forgive them all, except Galen, *qui videtur nosse omnino debuisse*—"but, as for Galen, I know not at all what I am to think. For he, who made more than six hundred sections of living animals, as he boasts himself, and so often opened many animals when they were lately fed, are we to think it possible that these veins never showed themselves to him, that he never had them under his eyes, that he never investigated them—he to whom Erasistratus had given so great cause for searching out the whole matter?" Probably, the milk-white threads had been taken for nerves by those who had seen them:

and those who had never seen them, but believed in their existence, rested their belief on a general idea that the chyle must, somehow, have vessels of its own apart from the blood-vessels. What Galen and Erasistratus must have seen, Asellius and Pecquet discovered: and Harvey gives a careful review of the discovery in his letters to Nardi (May 1652) and to Morison (November 1653). He does not accept it; but the point is that he recognises it as a new thing altogether.

A year or two after he had made the discovery, Asellius died; and his work was published in 1627 by two Milanese physicians, and was dedicated by them to the senate of the Academy of Milan, where Asellius had been professor of anatomy. The full title of his book is, *De Lactibus sive Lacteis Venis, quarto Vasorum Mesaraicorum genere novo invento, Gasparis Asellii Cremonensis, Anatomici Ticinensis, Dissertatio. Quâ sententiæ anatomicæ multæ vel perperam receptæ convelluntur vel partim perceptæ illustrantur.* He gives the following account of the discovery, in the chapter entitled *Historia primæ vasorum istorum inventionis cum fide narrata.* On 23rd July 1622, demonstrating the movement of the diaphragm in a dog, he observed suddenly, "as it were, many threads, very thin and very white, dispersed through the whole mesentery and through the intestines, with ramifications almost endless"—*plurimos, eosque tenuissimos candidosissimosque ceu funiculos per omne mesenterium et per intestina infinitis propemodum propaginibus dispersos:*—

"Thinking at first sight that they were nerves, I did not greatly heed them. But soon I saw that I was wrong, for I bethought me that the nerves, which belong to the intestines, are distinct from these threads, and

very different from them, and have a separate course. Wherefore, struck by the newness of the matter, I stopped for a time silent, while one way and another there came to my mind the controversies that occupy anatomists, as to the mesenteric veins and their use; which controversies are as full of quarrels as of words. When I had pulled myself together, to make experiment, taking a very sharp scalpel, I pierce one of the larger threads. Scarcely had I hit it off, when I see a white fluid running out, like milk or cream. At which sight, when I could not hold my joy, turning to those who were there, first to Alexander Tadinus and Senator Septalius, both of them members of the most honourable College of Physicians, and, at the time of this writing, officers of the public health, '*I have found it*,' I say like Archimedes; and therewith invite them to the so pleasant sight of a thing so unwonted; they being agitated, like myself, by the newness of it."

He then describes the collapse and disappearance of the vessels at death, and the many experiments which he made for further study of them; and the failure, when he tried to find them in animals not lately fed. He did not trace them beyond the mesentery, and believed that they emptied themselves into the liver. The discovery of their connection with the receptaculum chyli and the thoracic duct was made by Jehan Pecquet of Dieppe, Madame de Sévigné's doctor, her "good little Pecquet." The full title of his book (2nd ed., 1654) is, *Experimenta Nova Anatomica, quibus incognitum hactenus Receptaculum, et ab eo per Thoracem in ramos usque subclavios Vasa Lactea deteguntur.* He has not the academical learning of Asellius, nor his obsequious regard for the ancients; and the discovery of the thoracic duct came, as it were by chance, out of an experiment that was of itself wholly useless. He

had killed an animal by removing its heart, and then saw a small quantity of milky fluid coming from the cut end of the vena cava—*Albicantem subinde Lactei liquoris, nec certe parum fluidi scaturiginem, intra Venæ Cavæ fistulam, circā dextri sedem Ventriculi, miror effluere*—and found that this fluid was identical with the chyle in the lacteals. In another experiment, he succeeded in finding the thoracic duct—"At last, by careful examination deep down along the sides of the dorsal vertebræ, a sort of whiteness, as of a lacteal vessel, catches my eyes. It lay in a sinuous course, close up against the spine. I was in doubt, for all my scrutiny, whether I had to do with a nerve or with a vessel. Therefore, I put a ligature a little below the clavicular veins; and then the flaccidity above the ligature, and the swelling of the distended duct below the ligature, broke down my doubt—*Ergo subducto paulo infra Claviculas vinculo, cum a ligaturâ sursum flaccesceret, superstite deorsum turgentis alveoli tumore, dubium meum penitus enervavit. . . . Laxatis vinculis, lacteus utrinque rivulus in Cavam affatim Chylum profudit.*"

It is to be noted that Asellius and Pecquet, both of them, made their discoveries as it were by chance. Unless digestion were going on, the lacteals would be empty and invisible; and, on the dead body, lacteals, receptaculum, and thoracic duct would all be empty. For these reasons, it cost a vast number of experiments to prove the existence, and to discover the course, of these vessels. Once found in living animals, they could be injected and dissected in the dead body; but they had been overlooked by Vesalius and the men of his time.

From the discovery of the lacteals came the discovery of the whole lymphatic system. Daremberg, in his *Histoire des Sciences Médicales* (Paris, 1870), after an account of Pecquet's work, says :—

"Up to this point, we have seen English, Italians, and French working together, with more or less success and genius, to trace the true ways of blood and chyle : there is yet one field of work to open up, the lymphatics of the body. The chief honour here belongs, without doubt, to the Swede Rudbeck, though the Dane Bartholin has disputed it with him, with equal acrimony and injustice."

Rudbeck's work (1651–54) coincides exactly, in point of time, with the first and second editions, 1651 and 1654, of Pecquet's *De Lactibus*. It may be said, therefore, that the whole doctrine of the lymphatic system was roughed out half-way through the seventeenth century.

III

THE GASTRIC JUICE

From many causes, the experimental study of the digestive processes came later than the study of the circulation. As an object of speculative thought, digestion was a lower phase of life, the work of crass spirits, less noble than the blood; from the point of view of science, it could not be studied ahead of organic chemistry, and got no help from any other sort of knowledge; and, from the medical point of view, it was the final result of many unknown internal forces that could not be observed or estimated either in life or after death. It did not, like the circulation, centre itself round one problem; it could not be focussed by the work of one man. For these reasons, and especially because of its absolute dependence on chemistry for the interpretation of its facts, it had to bide its time; and Réaumur's experiments are separated from the publication of Harvey's *De Motu Cordis et Sanguinis* by a hundred and thirty years.

The following account of the first experiments on digestion is taken from Claude Bernard's *Physiologie Opératoire*, 1879:—

"The true experimental study of digestion is of comparatively recent date; the ancients were content to find comparisons, more or less happy, with common

facts. Thus, for Hippocrates, digestion was a 'coction': for Galen, a 'fermentation,' as of wine in a vat. In later times, van Helmont started this comparison again: for him, digestion was a fermentation like that of bread: as the baker, having kneaded the bread, keeps a little of the dough to leaven the next lot kneaded, so, said van Helmont, the intestinal canal never completely empties itself, and the residue that it keeps after each digestion becomes the leaven that shall serve for the next digestion.

"The first experimental studies on the digestion date from the end of the seventeenth century, when the Academy of Florence was the scene of a famous and long controversy between Borelli and Valisnieri. The former saw nothing more in digestion than a purely mechanical act, a work of attrition whereby the ingesta were finely divided and as it were pulverised: and in support of this opinion Borelli invoked the facts that he had observed relating to the gizzard of birds. We know that this sac, with its very thick muscular walls, can exercise on its contents pressure enough to break the hardest bodies. Identifying the human stomach with the bird's gizzard, Borelli was led to attribute to the walls of the stomach an enormous force, estimated at more than a thousand pounds; whose action, he said, was the very essence of digestion. Valisnieri, on the contrary, having had occasion to open the stomach of an ostrich, had found there a fluid which seemed to act on bodies immersed in it; this fluid, he said, was the active agent of digestion, a kind of *aqua fortis* that dissolved food.

"These two opposed views, resulting rather from observations than from regularly instituted experiments, were the starting-point of the experimental researches undertaken by Réaumur in 1752. To resolve the problem set by Borelli and Valisnieri, Réaumur made birds swallow food enclosed in fenestrated tubes, so that the food, protected from the mechanical action of the walls of the stomach, was yet exposed to

the action of the gastric fluid. The first tubes used (glass, tin, etc.) were crushed, bent, or flattened by the action of the walls of the gizzard; and Réaumur failed to oppose to this force a sufficient resistance, till he employed leaden tubes thick enough not to be flattened by a pressure of 484 pounds: which was, in fact, the force exercised by the contractile walls of the gizzard in turkeys, ducks, and fowls under observation. These leaden tubes—filled with ordinary grain, and closed only by a netting that let pass the gastric juices—these tubes, after a long stay in the stomach, still enclosed grain wholly intact, unless it had been crushed before the experiment. When they were filled with meat, it was found changed, but not digested. Réaumur was thus led at first to consider digestion, in the gallinaceæ, as pure and simple trituration. But, repeating these experiments on birds of prey, he observed that digestion in them consists essentially in dissolution, without any especial mechanical action, and that it is the same with the digestion of meat in all animals with membranous stomachs. To procure this dissolving fluid, Réaumur made the birds swallow sponges with threads attached: withdrawing these sponges after a definite period, he squeezed the fluid into a glass, and tested its action on meat. That was the first attempt at artificial digestion *in vitro*. He did not carry these last investigations very far, and did not obtain very decisive results; nevertheless he must be considered as the discoverer of artificial digestion."

After Réaumur, the Abbé Spallanzani (1783) made similar observations on many other animals, including carnivora. He showed that even in the gallinaceæ there was dissolution of food, not mere trituration: and observed how after death the gastric fluid may under certain conditions act on the walls of the stomach itself.

"Henceforth the experimental method had cut the knot of the question raised by the theories of Borelli and Valisnieri: digestion could no longer be accounted anything but a dissolution of food by the fluid of the stomach, the gastric juice. But men had still to understand this gastric juice, and to determine its nature and mode of action. Nothing could be more contradictory than the views on this matter. Chaussier and Dumas, of Montpellier, regarded the gastric juice as of very variable composition, one time alkaline, another acid, according to the food ingested. Side by side with these wholly theoretical opinions, certain results of experiments had led to ideas just as erroneous, for want of rigorous criticism of methods; it was thus that Montègre denied the existence of the gastric juice as a special fluid; what men took for gastric juice, he said, was nothing but the saliva turned acid in the stomach. To prove his point, he made the following experiment:—He masticated a bit of bread, then put it out on a plate; it was at first alkaline, then at the end of some time it became acid. In those days (1813) this experiment was a real embarrassment to the men who believed in the existence of a special gastric juice: we have now no need to refute it.

"These few instances suffice to show how the physiologists were unsettled as to the nature and properties of the gastric juice. Then (1823) the Academy had the happy idea of proposing digestion as a subject for a prize. Tiedemann and Gmelin in Germany, Leuret and Lassaigne in France, submitted works of equal merit, and the Academy divided the prize between them. The work of Tiedemann and Gmelin is of especial interest to us on account of the great number of their experiments, from which came not only the absolute proof of the existence of the gastric juice, but also the study of the transformation of starch into glucose. Thus the theory of digestion entered a new phase: it was finally recognised, at least for certain substances, that digestion is not simply dissolution, but a true chemical transformation." (Cl. Bernard, *loc. cit.*)

IV

GLYCOGEN

CLAUDE BERNARD'S discovery of glycogen in the liver had a profound influence both on physiology and on pathology. Take first its influence on pathology. Diabetes was known to Celsus, Aretæus, and Galen; Willis, in 1674, and Morton, in 1675, noted the distinctive sweetness of the urine; and their successors proved the presence of sugar in it. Rollo, in 1787, observed that vegetable food was bad for diabetic patients, and introduced the strict use of a meat diet. But Galen had believed that diabetes was a disease of the kidneys, and most men still followed him: nor did Rollo greatly advance pathology by following not Galen, but Aretæus. Later, with the development of organic chemistry, came the work of Chevreuil (1815), Tiedemann and Gmelin (1823), and other illustrious chemists: and the pathology of diabetes grew more and more difficult:—

"These observations gave rise to two theories: the one, that sugar is formed with abnormal rapidity in the intestine, absorbed into the blood, and excreted in the urine; the other, that diabetes is due to imperfect destruction of the sugar, either in the intestine or in the blood. Some held that it underwent conversion into lactic acid as it was passing through the intestinal walls, while others believed it to be destroyed in the blood by means of the alkali therein contained."[1]

[1] *Reynolds' System of Medicine*, vol. v., art. "Diabetes Mellitus."

Thus, before Claude Bernard (1813–1878), the pathology of diabetes was almost worthless. And, in physiology, his work was hardly less important than the work of Harvey. A full account of it, in all its bearings, is given in Sir Michael Foster's *Life of Claude Bernard* (Fisher Unwin, 1899).

In Bernard's *Leçons sur le Diabète et la Glycogenèse Animale* (Paris, 1877), there is a sentence that has been misquoted many times:—

Sans doute, nos mains sont vides aujourd'hui, mais notre bouche peut être pleine de légitimes promesses pour l'avenir.

This sentence has been worked so hard that some of the words have got rubbed off it: and the statement generally made is of this kind:—

Claude Bernard himself confessed that his hands were empty, but his mouth was full of promises.

Of course, he did not mean that he was wrong in his facts. But, in this particular lecture, he is speaking of the want of more science in practice, looking forward to a time when treatment should be based on science, not on tradition. Medicine, he says, is neither science nor art. Not science—*Trouverait-on aujourd'hui un seul médecin raisonnable et instruit osant dire qu'il prévoit d'une manière certaine la marche et l'issue d'une maladie ou l'effet d'une remède?* Not art, because art has always something to show for its trouble: a statue, a picture, a poem—*Le médecin artiste ne crée rien, et ne laisse aucune œuvre d'art, à moins d'appliquer ce titre à la guérison du malade. Mais quand le malade meurt, est-ce également son œuvre? Et quand il guérit, peut-il distinguer sa part de celle de la nature?*

To Claude Bernard, experiments on animals for the

direct advancement of medicine seemed a new thing: new, at all events, in comparison with the methods of some men of his time. He was only saying what Sir John Burdon Sanderson said in 1875 to the Royal Commission:—

It is my profound conviction that a future will come, it may be a somewhat distant future, in which the treatment of disease will be really guided by science. Just as completely as mechanical science has come to be the guide of the mechanical arts, do I believe, and I feel confident, that physiological science will eventually come to be the guide of medicine and surgery.

Anyhow, lecturing a quarter of a century ago on diabetes, his special subject, Claude Bernard spoke out his longing to compel men into the ways of science, to give them some immediate sign which they could not refuse to see:—

"At this present time, medicine is passing from one period to another. The old traditions are losing ground, and scientific medicine (*la médecine expérimentale*) has got hold of all our younger men: every day it gains ground, and will establish itself against all its critics, and in spite of the excesses of those who are over-zealous for its honour. . . . And when men ask us what are the results of scientific medicine, we are driven to answer that it is scarcely born, that it is still in the making. Those who care for nothing but an immediate practical application must remember Franklin's words, *What is the use of a new-born child, but to become a man?* If you deliberately reject scientific medicine, you fail to see the natural development of man's mind in all the sciences. Without doubt, our hands are empty to-day, but our mouth may well be filled with legitimate promises for the future."

He died in 1878. The following account of the discovery of glycogen is taken from his *Nouvelle Fonction du Foie* (Paris, 1853):—

"My first researches into the assimilation and destruction of sugar in the living organism were made in 1843: and in my inaugural thesis (Dec. 1843) I published my first experiments on the subject. I succeeded in demonstrating a fact hitherto unknown, that cane-sugar cannot be directly destroyed in the blood. If you inject even a very small quantity of cane-sugar, dissolved in water, into the blood or under the skin of a rabbit, you find it again in the urine unchanged, with all its chemical properties the same. . . . I had soon to give up my first point of view, because this question of the existence of a sugar-producing organ, that I had thought such a hard problem of physiology, was really the first thing revealed to me, as it were of itself, at once."

He kept two dogs on different diets, one with sugar, the other without it; then killed them during digestion, and tested the blood in the hepatic veins:—

"What was my surprise, when I found a considerable quantity of sugar in the hepatic veins of the dog that had been fed on meat only, and had been kept for eight days without sugar: just as I found it in the other dog that had been fed for the same time on food rich in sugar. . . .

"Finally, after many attempts—*après beaucoup d'essais et plusieurs illusions que je fus obligé de rectifier par des tâtonnements*—I succeeded in showing, that in dogs fed on meat the blood passing through the portal vein does not contain sugar before it reaches the liver; but when it leaves the liver, and comes by the hepatic veins into the inferior vena cava, this same blood contains a considerable quantity of a sugary substance (glucose)."

His further discovery, that this formation of sugar is increased by puncture of the floor of the fourth ventricle,

was published in 1849. It is impossible to exaggerate the importance of Claude Bernard's single-handed work in this field of physiology and pathology:—

"As a mere contribution to the history of sugar within the animal body, as a link in the chain of special problems connected with digestion and nutrition, its value was very great. Even greater, perhaps, was its effect as a contribution to general views. The view that the animal body, in contrast to the plant, could not construct, could only destroy, was, as we have seen, already being shaken. But evidence, however strong, offered in the form of numerical comparisons between income and output, failed to produce anything like the conviction which was brought home to every one by the demonstration that a substance was actually formed within the animal body, and by the exhibition of the substance so formed.

"No less revolutionary was the demonstration that the liver had other things to do in the animal economy besides secreting bile. This, at one blow, destroyed the then dominant conception that the animal body was to be regarded as a bundle of organs, each with its appropriate function, a conception which did much to narrow inquiry, since when a suitable function had once been assigned to an organ there seemed no need for further investigations. . . .

"No less pregnant of future discoveries was the idea suggested by this newly-found-out action of the hepatic tissue, the idea happily formulated by Bernard as 'internal secretion.' No part of physiology is at the present day being more fruitfully studied than that which deals with the changes which the blood undergoes as it sweeps through the several tissues, changes by the careful adaptation of which what we call the health of the body is secured, changes the failure or discordance of which entails disease. The study of these internal secretions constitutes a path of inquiry which has already been trod with conspicuous success, and which promises to lead to

untold discoveries of the greatest moment; the gate to this path was opened by Bernard's work." (Sir M. Foster, *loc. cit.*)

But the work to be done, before all the clinical facts of the disease can be stated in terms of physiology, is not yet finished. In England, especial honour is due to Dr. Pavy for his life-long study of this most complex problem.

V

THE PANCREAS

HERE again Claude Bernard's name must be put first. Before him, the diverse actions of the pancreatic juice had hardly been studied. Vesalius, greatest of all anatomists, makes no mention of the duct of the pancreas, and speaks of the gland itself as though its purpose were just to support the parts in its neighbourhood—*ut ventriculo instar substerniculi ac pulvinaris subjiciatur.* The duct was discovered by Wirsung, in 1642: but anatomy could not see the things that belong to physiology. Lindanus (1653) said, *I cannot doubt that the pancreas expurgates, in the ordinary course of Nature, those impurities of the blood that are too crass and inept to be tamed by the spleen: and, in the extraordinary course, all black bile, begotten of disease or intemperate living.* Wharton (1656) said, *It ministers to the nerves, taking up certain of their superfluities, and remitting them through its duct into the intestines.* And Tommaso Bartholini (1666) called it the *biliary vesicle of the spleen.*

This chaos of ideas was brought into some sort of order by Regnier de Graaf, pupil of François de Bois (Sylvius). De Bois had guessed that the pancreas must be considered not according to its position in the body, but according to its structure: that it was analogous to the salivary glands. He urged his pupil to make experiments on it: and de Graaf says:—

"I put my hand to the work: and though many times I despaired of success, yet at last, by the blessing of God on my work and prayers, in the year 1660 I discovered a way of collecting the pancreatic juice."

And, by further experiment, he refuted Bartholini's theory that the pancreas was dependent on the spleen.

Sylvius had supposed that the pancreatic juice was slightly acid, and de Graaf failed to note this mistake; but it was corrected by Bohn's experiments in 1710.

Nearly two hundred years come between Regnier de Graaf and Claude Bernard: it is no wonder that Sir Michael Foster says that de Graaf's work was "very imperfect and fruitless." So late as 1840, there was yet no clear understanding of the action of the pancreas. Physiology could not advance without organic chemistry; de Graaf could no more discover the amylolytic action of the pancreatic juice than Galvani could invent wireless telegraphy. The physiologists had to wait till chemistry was ready to help them:—

"Of course, while physical and chemical laws were still lost in a chaos of undetermined facts, it was impossible that men should analyse the phenomena of life: first, because these phenomena go back to the laws of chemistry and physics; and next, because they cannot be studied without the apparatus, instruments, and all other methods of analysis that we owe to the laboratories of the chemists and the physicists." (Cl. Bernard, *Phys. Opér.*, p. 61.)

Therefore de Graaf failed, because he got no help from other sciences. But it cannot be called failure;

he must be contrasted with the men of his time, Lindanus and Bartholini, facts against theories, not with men of this century. And Claude Bernard went back to de Graaf's method of the fistula, having to guide him the facts of chemistry observed by Valentin, Tiedemann and Gmelin, and Eberlé. His work began in 1846, and the Académie des Sciences awarded a prize to it in 1850:—

"Let this vague conception (the account of the pancreas given in Johannes Müller's Text-book of Physiology) be compared with the knowledge which we at present have of the several distinct actions of the pancreatic juice, and of the predominant importance of this fluid not only in intestinal digestion but in digestion as a whole, and it will be at once seen what a great advance has taken place in this matter since the early forties. That advance we owe in the main to Bernard. Valentin, it is true, had in 1844 not only inferred that the pancreatic juice had an action on starch, but confirmed his view by actual experiment with the juice expressed from the gland; and Eberlé had suggested that the juice had some action on fat; but Bernard at one stroke made clear its threefold action. He showed that it on the one hand emulsified, and on the other hand split up, into fatty acids and glycerine, the neutral fats; he clearly proved that it had a powerful action on starch, converting it into sugar; and lastly, he laid bare its remarkable action on proteid matters." (Sir Michael Foster, *loc. cit.*)

Finally came the discovery that the pancreas—apart from its influences on digestion—contributes its share, like the ductless glands, to the general chemistry of the body:—

"It was discovered, a few years ago, by von Mering and Minkowski, that if, instead of merely diverting its

secretion, the pancreas is bodily removed, the metabolic processes of the organism, and especially the metabolism of carbo-hydrates, are entirely deranged, the result being the production of permanent diabetes. But if even a very small part of the gland is left within the body, the carbo-hydrate metabolism remains unaltered, and there is no diabetes. The small portion of the organ which has been allowed to remain (and which need not even be left in its proper place, but may be transplanted under the skin or elsewhere) is sufficient, by the exchanges which go on between it and the blood generally, to prevent those serious consequences to the composition of the blood, and the general constitution of the body, which result from the complete removal of this organ." (Prof. Schäfer, 1894.)

Here, in this present study of "pancreatic diabetes," by Dr. Vaughan Harley and others, are facts as important as any that Bernard made out: in no way contradicting his work, but adding to it. The pancreas is no longer taken to be only a sort of salivary gland out of place: over and above the secretion that it pours into the intestines, it has an "internal secretion," a constituent of the blood: it belongs not only to the digestive system, but also, like the thyroid gland and the supra-renal capsules, to the whole chemistry of the blood and the tissues. So far has physiology come, unaided by anatomy, from the fantastic notions of Lindanus and the men of his time: and has come every inch of the way by the help of experiments on animals. Professor Starling's observations, on the chemical influence of the duodenal mucous membrane on the flow of pancreatic fluid, have advanced the subject still further.

VI

THE GROWTH OF BONE

THE work of du Hamel proved that the periosteum is one chief agent in the growth of bone. Before him, this great fact of physiology was unknown; for the experiments made by Anthony de Heide (1684), who studied the production of callus in the bones of frogs, were wholly useless, and serve only to show that men in his time had no clear understanding of the natural growth of bone. De Heide says of his experiments:—

"From these experiments it appears—*forsan probatur*—that callus is generated by extravasated blood, whose fluid particles being slowly exhaled, the residue takes the form of the bone: which process may be further advanced by deciduous halitus from the ends of the broken bone."

And Clopton Havers, in his *Osteologia Nova* (London, 1691), goes so far the wrong way that he attributes to the periosteum not the production of bone, but the prevention of over-production; the periosteum, he says, is put round the shaft of a bone to compress it, lest it grow too large.

Du Hamel's discovery (1739–1743) came out of a chance observation, made by John Belchier,[1] that the

[1] "An Account of the Bones of Animals being changed to a Red Colour by Aliment only," by John Belchier, F.R.S., *Phil. Trans. Roy. Soc.*, 1735–36. There is a letter from Sir Hans Sloane, then President of the Royal Society, to M. Geoffroy, member of the

bones of animals fed near dye-works were stained with the dye. Belchier therefore put a bird on food mixed with madder, and found that its bones had taken up the stain. Then du Hamel studied the whole subject by a series of experiments. To estimate the advance that he gave to physiology, contrast de Heide's fanciful language with the title of one of du Hamel's papers—*Quatrième Mémoire sur les Os, dans lequel on se propose de rapporter de nouvelles preuves qui établissent que les os croissent en grosseur par l'addition de couches osseuses qui tirent leur origine du périoste, comme le corps ligneux des Arbres augmente en grosseur par l'addition de couches ligneuses qui se forment dans l'écorce.* Or take an example of du Hamel's method :—

"Three pigs were destined to clear up my doubts. The first, six weeks old, was fed for a month on ordinary food, with an ounce daily of madder-juice—*garence grappe*—put in it. At the end of the month, we stopped the juice, and fed the pig in the ordinary way for six weeks, and then killed it. The marrow of the bones was surrounded by a fairly thick layer of white bone : this was the formation of bone during the first six weeks of life, without madder. This ring of white bone was surrounded by another zone of red bone : this was the formation of bone during the administration of the madder. Finally, this red zone was covered with a fairly thick layer of

French Academy :—"M. Belchier, chirurgien, membre de cette Société, dînant un jour chez un Teinturier qui travaille en Toiles peintes, remarqua que dans un Porc frais qu'on avoit servi sur table, et dont la chair étoit de bon goût, les os étoient rouges. Il demanda la cause d'un effet si singulier, et on lui dit que ces sortes de Teinturiers se servoient de la racine de Rubia Tinctorum, ou garence, pour fixer les couleurs déjà imprimées sur les Toiles de coton, qu'on appelle en Angleterre callicoes." This passage of dye into the bones of animals had been noted so far back as 1573, by Antoine Mizald, a doctor in Paris—*Erythrodanum, vulgo rubia tinctorum, ossa pecudum rubenti et sandycino colore imbuit.*

white bone: this was the layer formed after the madder had been left off. . . . We shall have no further difficulty in understanding whence transudes the osseous juice that was thought necessary for the formation of callus and the filling-up of the wounds of the bones, now we see that it is the periosteum that fills up the wounds, or is made thick round the fractures, and afterward becomes of the consistence of cartilage, and at last acquires the hardness of bones."

These results, confirmed by Bazan (1746) and Boehmer (1751), were far beyond anything that had yet been known about the periosteum. But the growth of bone is a very complex process: the naked eye sees only the grosser changes that come with it; and du Hamel's ingenious comparison between the periosteum and the bark of trees was too simple to be exact. Therefore his work was opposed by Haller, and by Dethleef, Haller's pupil: and the great authority of Haller's name, and the difficulties lying beyond du Hamel's plain facts, brought about a long period of uncertainty. Bordenave (1756) found reasons for supporting Haller; and Fougeroux (1760) supported du Hamel. Thus men came to study the whole subject with more accuracy—the growth in length, as well as the growth in thickness; the medullary cavity, the development of bone, the nutrition and absorption of bone. Among those who took up the work were Bichat, Hunter, Troja, and Cruveilhier; and they recognised the surgical aspect of these researches in physiology. After them, the periosteal growth of bone became, as it were, a part of the principles of surgery. From this point of view of practice, issued the experiments made by Syme (1837) and Stanley (1849): which proved the importance of the epiphysial cartilages for the growth of the

bones in length, and the risk of interfering with these cartilages in operations on the joints of children. Finally, with the rise of anæsthetics and of the antiseptic method, came the work of Ollier, of Lyon, whose good influence on the treatment of these cases can hardly be over-estimated.

VII

THE NERVOUS SYSTEM

As with the circulatory system, so with the nervous system, the work of Galen was centuries ahead of its time. Before him, Aristotle, who twice refers to experiments on animals, had observed the brain during life: for he says, "In no animal has the blood any feeling when it is touched, any more than the excretions; nor has the brain, or the marrow, any feeling, when it is touched": but there is reason for believing that he neither recognised the purpose of the brain, nor understood the distribution of the nerves. Galen, by the help of the experimental method, founded the physiology of the nervous system:—

"Galen's method of procedure was totally different to that of an anatomist alone. He first reviewed the anatomical position, and by dissection showed the continuity of the nervous system, both central and peripheral, and also that some bundles of nerve fibres were distributed to the skin, others to the muscles. Later, by process of the physiological experiment of dividing such bundles of fibres, he showed that the former were sensory fibres and the latter motor fibres. He further traced the nerves to their origins in the spinal cord, and their terminations as aforesaid. From these observations and experiments he was able to deduce the all-important fact that different nerve-roots supplied different groups of muscles and different areas of the skin. . . . An excellent illustration of his method, and of the fact that we ought not to treat symptoms, but the causes of symptoms, is shown very

clearly in one of the cases which Galen records as having come under his care. He tells us that he was consulted by a certain sophist called Pausanias, who had a severe degree of anæsthesia of the little and ring fingers. For this loss of sensation, etc., the medical men who attended him applied ointments of various kinds to the affected fingers; but Galen, considering that that was a wrong principle, inquired into the history, and found that while the patient was driving in his chariot he had accidentally fallen out and struck his spine at the junction of the cervical and dorsal regions. Galen recognised that he had to do with a traumatism affecting the eighth cervical and first dorsal nerve; therefore, he says, he ordered that the ointments should be taken off the hand and placed over the spinal column, so as to treat the really affected part, and not apply remedies to merely the referred seat of pain."[1]

Galen, by this sort of work, laid the foundations of physiology; but the men who came after him let his facts be overwhelmed by fantastic doctrines: all through the ages, from Galen to the Renaissance, no great advance was made toward the interpretation of the nervous system. Long after the Renaissance, his authority still held good; his ghost was not laid even by Paracelsus and Vesalius, it haunted the medical profession so late as the middle of the seventeenth century; but the men who worshipped his name missed the whole meaning of his work. This long neglect of the experimental method left such a gap in the history of physiology, that Sir Charles Bell seems to take up the experimental study of the nervous system at the point where Galen had stopped short; we go from the

[1] From an address on Galen, given by Sir Victor Horsley before the Medical Society of the Middlesex Hospital. See *Middlesex Hospital Journal*, May 1899.

time of Commodus to the time of George the Third, and there is Bell, as it were, putting the finishing touch to Galen's facts. It is true that experiments had been made on the nervous system by many men; but a dead weight of theories kept down the whole subject. For a good instance, how imagination hindered science, there is the following list, made by Dr. Risien Russell, of theories about the cerebellum:—

"Galen was of opinion that the cerebellum must be the originator of a large amount of vital force. After him, and up to the time of Willis, the prevalent idea seems to have been that it was the seat of memory; while Bourillon considered it the seat of instinct and intelligence. Willis supposed that it presided over involuntary movements and organic functions; and this view, though refuted by Haller, continued in the ascendency for some time. Some believed strongly in its influence on the functions of organic life; and according to some, diseases of the cerebellum appeared to tell on the movements of the heart. . . . Haller believed it to be the seat of sensations, as well as the source of voluntary power; and there were many supporters of the theory that the cerebellum was the seat of the sensory centres. Renzi considered this organ the nervous centre by which we perceive the reality of the external world, and direct and fix our senses on the things round us. Gall, and later Broussais, and others, held that this organ presided over the instinct of reproduction, or the propensity to love; while Carus regarded it as the seat of the will also. Rolando looked on it as the source of origin of all movements. Jessen adduced arguments in favour of its being the central organ of feeling, or of the soul, and the principal seat of the sensations."

It is plain, from this list, that physiology had become obscured by fanciful notions of no practical value. If a better understanding of the nervous system could have

been got without experiments on animals, why had men to wait so long for it? The Italian anatomists had long ago given them all the anatomy that was needed to make a beginning; the hospitals, and practice, had given them many hundred years of clinical facts; nervous diseases and head injuries were common enough in the Middle Ages; and by the time of Ambroise Paré, if not before, *post-mortem* examinations were allowed. The one thing wanted was the experimental method; and, for want of it, the science of the nervous system stood still. Experiments had been made; but the steady, general, unbiassed use of this method had been lost sight of, and men were more occupied with logic and with philosophy.

Then, in 1811, came Sir Charles Bell's work. If any one would see how great was the need of experiments on animals for the interpretation of the nervous system, let him contrast the physiology of the eighteenth century with that one experiment by Bell which enabled him to say, "I now saw the meaning of the double connection of the nerves with the spinal marrow." It is true that this method is but a part of the science of medicine; that experiment and experience ought to go together like the convexity and the concavity of a curve. But it is true also that men owe their deliverance from ignorance about the nervous system more to experiments on animals than to any other method of observing facts.

1. *Sir Charles Bell* (1778–1842)

The great authority of Sir Charles Bell has been quoted a thousand times against all experiments on animals:—

"Experiments have never been the means of discovery; and a survey of what has been attempted of late years in physiology, will prove that the opening of living animals has done more to perpetuate error than to confirm the just views taken from the study of anatomy and natural motions."

He wrote, of course, in the days before bacteriology, before anæsthetics; he had in his mind neither inoculations, nor any observations made under chloroform or ether, but just "the opening of living animals." He had also in his mind, and always in it, a great dislike against the school of Magendie. Let all that pass; our only concern here is to know whether these words are true of his own work.

They occur in a paper, *On the Motions of the Eye, in Illustration of the Uses of the Muscles and Nerves of the Orbit;* communicated by Sir Humphry Davy to the Royal Society, and read March 20, 1823.[1] This essay was one of a series of papers on the nervous system, presented to the Royal Society during the years 1821–1829. In 1830, having already published four of these papers under the title, *The Exposition of the Nervous System*, Bell published all six of them, under the title, *The Nervous System of the Human Body.*

In his Preface to this book (1830) he quotes the earliest of all his printed writings on the nervous system, a pamphlet, printed in 1811, under the title, *An Idea of a New Anatomy of the Brain, Submitted for the Observation of the Author's Friends.* We have therefore two statements of his work, one in 1811, the other in 1823 and 1830. The first of them was written when his work was still new before his eyes.

[1] This paper includes an *Experimental Enquiry into the Action of these Muscles*, giving an account of an experiment on the eye.

Those who say that experiments did not help Bell in his great discovery—the difference between the anterior and the posterior nerve-roots—appeal to certain passages in the 1830 volume:—

"In a foreign review of my former papers, the results have been considered as a further proof in favour of experiments. They are, on the contrary, deductions from anatomy; and I have had recourse to experiments, not to form my own opinions, but to impress them upon others. It must be my apology that my utmost efforts of persuasion were lost, while I urged my statements on the grounds of anatomy alone. I have made few experiments; they have been simple and easily performed, and I hope are decisive. . . .

"My conceptions of this matter arose by inference from the anatomical structure; so that the few experiments which have been made were directed only to the verification of the fundamental principles on which the system is established."

If it were not for the 1811 pamphlet, the opponents of all experiments on animals might claim Sir Charles Bell on their side. But while his work was still a new thing, he spoke in another way of it:—

"I found that injury done to the anterior portion of the spinal marrow convulsed the animal more certainly than injury to the posterior portion; but I found it difficult to make the experiment without injuring both portions.

"Next, considering that the spinal nerves have a double root, and being of opinion that the properties of the nerves are derived from their connections with the parts of the brain, *I thought that I had an opportunity of putting my opinion to the test of experiment, and of proving at the same time* that nerves of different endowments were in the same cord (nerve-trunk) and held together by the same sheath.

"On laying bare the roots of the spinal nerves, I found that I could cut across the posterior fasciculus of nerves, which took its origin from the posterior portion of the spinal marrow, without convulsing the muscles of the back; but that on touching the anterior fasciculus with the point of the knife, the muscles of the back were immediately convulsed.

"*Such were my reasons for concluding* that the cerebrum and cerebellum were parts distinct in function, and that every nerve possessing a double function obtained that by having a double root. *I now saw the meaning* of the double connection of the nerves with the spinal marrow; and also the cause of that seeming intricacy in the connections of nerves throughout their course, which were not double at their origins."

It is impossible to reconcile the 1830 sentences with this vivid personal account of himself; *I had an opportunity of putting my opinion to the test of experiment . . . an opportunity of proving . . . Such were my reasons for concluding. . . . I now saw. . . .* It is just what all men of science say of their experiments: the very phrase of Archimedes, and Asellius, and de Graaf. If Sir Charles Bell had been working at the facts of chemistry or of botany, who would have doubted the meaning of these words?

This same inconsistency of sentences occurs elsewhere in his *Nervous System of the Human Body.* In one place he says that he has made few experiments: *They have been simple, and easily performed, and I hope are decisive.* In another he says: "*After making several experiments on the cerebrum and cerebellum, I laid the question of their functions entirely aside,* and confined myself to the investigation of the spinal marrow and the nerves; *a subject which I found more within my power,* and which forms the substance of the present volume."

Next, take his account of the cranial nerves :—

"It was necessary to know, in the first place, whether the phenomena exhibited on injuring the separate roots corresponded with what was suggested by their anatomy. . . .

"Here a difficulty arose. An opinion prevailed that ganglions were intended to cut off sensation; and every one of these nerves, which I supposed were the instruments of sensation, have ganglions on their roots. Some very decided experiment was necessary to overturn this dogma. (Account of the experiment.) By pursuing the inquiry, it was found that a ganglionic nerve is the sole organ of sensation in the head and face: ganglions were therefore no hindrance to sensation; and thus my opinion was confirmed. . . . *It now became obvious* why the third, sixth, and ninth nerves of the encephalon were single nerves in their roots. . . .

"Observing that there was a portion of the fifth nerve which did not enter the ganglion of that nerve, and being assured of the fact by the concurring testimony of anatomists, I conceived that the fifth nerve was in fact the uppermost nerve of the spine. . . . This opinion was confirmed by experiment. . . . (Account of an experiment on the dead body.) On dividing the root of the nerve in a living animal, the jaw fell relaxed. Thus its functions are no longer matter of doubt: it is at once a muscular nerve and a nerve of sensibility. And thus the opinion is confirmed, that the fifth nerve is to the head what the spinal nerves are to the other parts of the body, in respect to sensation and volition."

The value of the experimental method could hardly be stated in more emphatic words. He supposed something, conceived it, had an opinion about it. Anatomy had suggested something to him. He put his opinion to the test of phenomena, that is to say, to the test of visible facts; and then his opinion was confirmed. As

with the spinal nerve-roots, so with the fifth cranial nerve—his work was successful, because he followed the way of experiment.

He was by nature of a most complex and sensitive temperament, full of contrary forces—one man in 1811, another in 1830. In 1811 he wrote, *I now saw the meaning of the double connection of the nerves;* in 1830 he had come to hate the *stupid sterile materialism* of the French school: he beheld anatomy falling behind physiology, and his Windmill Street school perishing to make way for the Hospital schools and for the University of London. He was before everything else a great anatomist: he stood up for the honour of anatomy against the new physiology, and for the honour of the Monroes and the Hunters against Magendie: he hated the notion that any man should proceed to experiments on function till the very last secrets had been got out of structure. He died a few years afterward. The 1830 writings are his last stand for the defence of his country, his school, and his beloved anatomy, against the methods of Magendie; who said of himself, "I am a mere street scavenger, *chiffonier*, of science. With my hook in my hand and my basket on my back, I go about the streets of science, collecting what I find."

This open conflict between Bell's first and last thoughts is a part of his character: he was brilliant, impulsive, changeable, inconsistent; and, what is more important, his honour kept him from trying to evade this trumpery charge of inconsistency; and he reprinted the 1811 Preface in the book that he published in 1830. Doubtless he would have picked his words more carefully if he had foreseen that one of the 1830 sentences would be wrested out of its place in his life's

work, and used as false evidence against the very method that he followed.

His observations on the cranial nerves brought about an immediate change in the practice of surgery:—

"Up to the time that Sir Charles Bell made his experiments on the nerves of the face, it was the common custom of surgeons to divide the facial nerve for the relief of neuralgia, *tic douleureux;* whereas it exercises, and was proved by Sir Charles Bell to exercise, no influence over sensation, and its division consequently for the relief of pain was a useless operation." (Sir J. Erichsen.)

The relation of Magendie's work on the nerve-roots to Bell's work need not be considered here. The exact dates of Bell's observations are given by one of his pupils in the Preface to the 1830 volume. Magendie finally proved the sensory nature of the posterior nerve-roots: "The exact and full proof which he brought forward of the truth which Charles Bell had divined rather than demonstrated, that the anterior and posterior roots of spinal nerves have essentially different functions—a truth which is the very foundation of the physiology of the nervous system—is enough by itself to mark him as a great physiologist." (Sir M. Foster, *loc. cit.*)

2. *Marshall Hall* (1790–1857)

Reflex action had been studied long before the time of Marshall Hall. The Hon. Robert Boyle (1663) had observed the movements and actions of decapitated vipers, flies, silkworms, and butterflies. Similar observations were made on frogs, eels, and other lower animals, by Redi, Woodward, Stuart, Le Gallois, and Sir Gilbert Blane. According to Richet, it was Willis who first gave the name *reflex* to these movements.

It cannot be said that these first studies of reflex action did much for physiology. But the following translation from Prochaska (1800) shows how they cleared the way for Marshall Hall's work, by the proof that they gave of the liberation of nervous energy in the spinal cord :—

"These movements of animals after decapitation must needs be by consent and commerce betwixt the spinal nerves. For a decapitated frog, if it be pricked, not only draws away the part that is pricked, but also creeps and jumps; which cannot happen but by consent betwixt the sensory nerves and the motor nerves. The seat of which consent must needs be in the spinal cord, the only remaining portion of the sensorium. *And this reflexion of sensory impressions into motor impressions is not accomplished in obedience to physical laws alone—wherein the angle of reflexion is equal to the angle of incidence, and reaction to action—but it follows special laws as it were written by Nature on the spinal cord, which we can know only by their effects, but cannot fathom with the understanding.* But the general law, whereby the sensorium reflects sensory impressions into motor impressions, is the preservation of ourselves."

It was not possible, in 1800, to go further, or to put the facts of reflex action more clearly: but this fine sentence gives no hint of the truth that guided Marshall Hall—that the "consent and commerce" of reflex action are to be found at definite points or levels in the spinal cord ; that the cord no more "works as a whole" than the brain. The greatness of Marshall Hall's work lies in his recognition of the divisional action of the cord: he proved the existence of definite centres in it, he discovered the facts of spinal localisation, and thus foreshadowed the discovery of cerebral localisation. In his earlier writings (1823–33) he showed how the movements of the trunk and of the

limbs are only one sort of reflex action; how the larynx, the pharynx, and the sphincter muscles, all act by the "consent and commerce" of the spinal cord. Later, in 1837, he demonstrated the course of nerve-impulses along the cord from one level to another, the results of direct stimulation of the cord, and other facts of spinal localisation. He noted the different effects of opium and of strychnine on reflex action; and he extended the doctrines of reflex action beyond physiology to the convulsive movements of the body in certain diseases.

3. *Flourens* (1794–1867)

Beside his work on the nervous system, Flourens studied the periosteal growth of bone, and the action of chloroform;[1] but he is best known by his experiments on the respiratory centre and the cerebellum. The men who interpreted the nervous system followed the anatomical course of that system: first the nerve-roots, then the cord, then the medulla oblongata and the cerebellum, and last the cerebral hemispheres; a steady upward advance, from the observation of decapitated insects to the localisation of centres in the human brain. Flourens, by his work on the medulla oblongata, localised the respiratory centre, the nerve-cells for the reflex movements of respiration:—

"M. Flourens a circonscrit ce centre avec une scrupuleuse précision, et lui a donné le nom de *nœud vital*." (Cl. Bernard.)

[1] When Flourens died, Claude Bernard was appointed to his place in the French Academy; and, in the *Discours de Reception* (May 27, 1869), said, "It is twenty-two years since the discovery of anæsthesia by ether came to us from the New World, and spread rapidly over Europe. M. Flourens was the first man who showed that chloroform is more active than ether."

Afterward came the discovery of cardiac and other centres in the same portion of the nervous system. Flourens also showed that the cerebellum is concerned with the equilibration of the body, and with the co-ordination of muscular movements; that an animal, a few days old, deprived of sensation and consciousness by removal of the cerebral hemispheres, was yet able to stand and move forward, but, when the cerebellum was removed, its muscles lost all co-ordinate action. (*Recherches Expérimentales*, Paris, 1842.) And from his work, and the work of those who followed him, on the semicircular canals of the internal ear, came the evidence that these minute structures are the terminal organs of equilibration: that as the special senses have their terminal apparatus and their central apparatus, so the semicircular canals and the cerebellum are the terminal apparatus and the central apparatus of the sense of equilibrium.

4. *Claude Bernard* (1813–1878)

The discovery of the vaso-motor nerves, and of the control of the nervous system over the calibre of the arteries, was made by Claude Bernard at the outset of his work on the influence of the nervous system on the temperature.[1] The evidence of Professor Sharpey before the Royal Commission of 1875 shows how things had

[1] A full account of this discovery, and of its relation to the experiments of Brown Séquard, Waller, and Budge, is given by Sir Michael Foster in his life of Claude Bernard; and the question of priority between Bernard and Brown Séquard need not be considered here, for the experimental method was the only way open to either of them. For an account of the work done, before Bernard, in this field of physiology, see Prof. Stirling's admirable and learned monograph, *Some Apostles of Physiology* (Waterlow & Sons, London, 1902), p. 104.

been misjudged, before Bernard's time, in the light of "views taken from the Study of Anatomy and Natural Motions":—

"I remember that Sir Charles Bell gave the increased size of the vessels in blushing, and their fulness of blood, as an example of the increased action of the arteries in driving on the blood. It turns out to be just the reverse, inasmuch as it is owing to a paralysis of the nerves governing the muscular coats of the arteries."

Claude Bernard's first account of his work was communicated to the Société de Biologie in December 1851. The following description is taken from his *Leçons de Physiologie Opératoire:*—

"I will remind you how I was led to the discovery of the vaso-motor nerves. Starting from the clinical observation, made long ago, that in paralysed limbs you find at one time an increase of cold, and at another an increase of heat, I thought this contradiction might be explained by supposing that, side by side with the general action of the nervous system, the sympathetic nerve might have the function of presiding over the production of heat; that is to say, that in the case where the paralysed limb was chilled, I supposed the sympathetic nerve to be paralysed, as well as the motor nerves; while in the paralysed limbs that were not chilled, the sympathetic nerve had retained its function, the systemic nerves alone having been attacked.

"This was a theory, that is to say, an idea leading me to make experiments; and for these experiments I must find a sympathetic nerve-trunk of sufficient size, going to some organ that was easy to observe, and must divide this trunk to see what would happen to the heat-supply of the organ. You know that the rabbit's ear, and the cervical sympathetic nerve of this animal, offered us the required conditions. So I divided the nerve; and immediately my experiment gave the lie direct to my

theory—*Je coupai donc ce filet et aussitôt l'expérience donna à mon hypothèse le plus éclatant démenti.* I had thought that the section of the nerve would suppress the function of nutrition, of calorification, over which the sympathetic system had been supposed to preside, and would cause the hollow of the ear to become chilled; and here was just the opposite, a very warm ear, with great dilatation of its vessels.

"I need not remind you how I made haste to abandon my first theory, and gave myself to the study of this new state of things. And you know that here was the starting-point of all my researches into the vaso-motor and thermic system; and the study of this subject is become one of the richest fields of experimental physiology."

Waller, in 1853, studied the vaso-motor centre in the spinal cord; and Schiff, in 1856, found evidence of the existence of two kinds of vaso-motor nerves—those that constrict the vessels, and those that dilate them. This view was finally established in 1858 by Claude Bernard's experiments on the chorda tympani and the submaxillary gland.

The *Leçons de Physiologie Opératoire* were published in 1879. Twenty years later, Sir Michael Foster says of Bernard's work:—

"It is almost impossible to exaggerate the importance of these labours of Bernard on the vaso-motor nerves, since it is almost impossible to exaggerate the influence which our knowledge of the vaso-motor system, springing as it does from Bernard's researches as from its fount and origin, has exerted, is exerting, and in widening measure will continue to exert, on all our physiological and pathological conceptions, on medical practice, and on the conduct of human life. There is hardly a physiological discussion of any width in which we do not sooner or later come on vaso-motor questions. Whatever part of physiology we touch, be it the work done by

a muscle, be it the various kinds of secretive labour, be it the insurance of the brain's well-being in the midst of the hydrostatic vicissitudes to which the changes of daily life subject it, be it that maintenance of bodily temperature which is a condition of the body's activity; in all these, as in many other things, we find vaso-motor factors intervening. And if, passing the insecure and wavering line which parts health from illness, we find ourselves dealing with inflammation, or with fever, or with any of the disordered physiological processes which constitute disease, we shall find, whatever be the tissue specially affected by the morbid conditions, that vaso-motor influences have to be taken into account. The idea of vaso-motor action is woven as a dominant thread into all the physiological and pathological doctrines of to-day; attempt to draw out that thread, and all that would be left would appear as a tangled heap."

5. *Cerebral Localisation*

Finally, moving upward along the anatomy of the nervous system, physiology came to study the motor-centres and special sense-centres of the cerebral hemispheres. The year 1861 may fairly be said to mark the beginning of the discovery of these centres, when Broca, at a meeting of the Anthropological Society of Paris, heard Aubertin's paper on the connection be tween the frontal convolutions and the faculty of speech But, of course, some sort of belief in cerebral localisation had been in the air long before Broca's time. Willis (1621–1675), who was contemporary with Sir Isaac Newton, had written of the brain as though its convolutions, or "cranklings" as he called them, showed that its work was departmental:—

"As the animal spirits for the various acts of imagination and memory ought to be moved within certain and

distinct limits, or bounded places, and these motions to be often iterated or repeated through the same tracts or paths, for that reason these manifold convolutions and infoldings of the brain are required for these divers manners of ordinations of the animal spirits—to wit, that in these cells or storehouses, severally placed, might be kept the species of sensitive things, and as occasion serves, may be taken from thence."[1]

And Gall, a century after Willis, had collected and published, in support of his system of phrenology, many cases and *post-mortem* examinations showing the differentiation of the work of the brain. Gall is a warning for all time against the dangers of deduction; he had but one idea, and he drove it to death; but the clinical and pathological facts which he amassed, in the hope of establishing a set of doctrines out of all relation to facts, are as true now as ever; and, if he had been content to go the way of induction, and to set himself to the accumulation of facts, he might have become a great physiologist. In his knowledge of the anatomy of the brain, and in the dissection of the brain, he was far ahead of the men of his time; but he followed his own imaginings, and left nothing that could last, except those cases and pathological instances that are buried in the ruins of his system. But there they are, and are still of value. For example, Gall's case of loss of speech, after an injury involving the speech-centres, ought to have commanded the attention of all physiologists: but it came to nothing, because he used it to support his doctrine of organs and bumps, and it shared the fate of that doctrine. Phrenology is gone

[1] For an account of Willis' work on the nervous system, see Sir Victor Horsley's *Fullerian Lectures*, 1891. Willis was the first, or one of the first, to recognise the fact that the cerebral ventricles are nothing more than lymph-cavities.

past recall; it died of that congenital disease, the deductive fallacy; but there was a time when it might have been turned to the service of science.

The excitement that Gall aroused by the spread of his ideas shows that some belief in cerebral centres was waiting for development. All men are by nature phrenologists; the commonplace excuses that are offered for lapses of memory, venial offences, and inherited weaknesses, all appeal to the comfortable notion that the offender is not wholly perverted, and that some very small and strictly localised group of cells is at fault. And it is probable that the physiology of the central nervous system, with its present strong tendency toward psychology, will some day be back, at a far higher level, above the point where phrenology went wrong. As Mme. de Staël said, *L'esprit humain fait progrès toujours, mais c'est progrès en spirale.* But the question, whether the general desire for a rational system of psychology will ever commend itself to physiology, belongs to the future. All that is of present concern is the steady, continuous, and successful advance, by the way of induction, and by the help of experiments on animals, toward a clear and accurate statement of the departmental work of the brain.

It is one of many instances how science and practice work together, that the modern study of these centres began not in experiment but in experience. The first centres that were thus studied were the speech-centres; and the observation of them arose out of the cases recorded by Bouillard in 1825, and Dax in 1836. Clinical observation, and *post-mortem* examination, found the speech-centres; physiological experiments had nothing to do with it; and phrenology had, as

it were, found them, and then lost them. But at once, so soon as practice gave the word to science, physiology set to work. These clinical facts had been there all the time; loss of speech had gone with disease or injury of "Broca's convolution" ever since man had been on the earth, and nobody had seen the significance of this sequence. Then, after 1861, everything was changed; and in a few years physiology had mapped out a large part of the surface of the brain, and had charted the motor-centres.

The story of Broca's convolution is told in Hamilton's *Text-Book of Pathology*:—

"In 1825, Bouillard collected a series of cases to show that the faculty of speech resided in the frontal lobes. In the year 1836 M. Dax, in a paper read to the Medical Congress of Montpellier, stated as a result of his researches that, where speech was lost from cerebral causes, he believed the lesion was invariably found in the left cerebral hemisphere, and that the accompanying paralysis of the right side of the body is consequent upon this. This paper for long lay buried in the annals of medical literature, but was unearthed years afterwards by his son, and presented to the French Academy. Bouillard's views were also disinterred by Aubertin, and in the year 1861 were brought by him before the notice of the Anthropological Society of Paris. Broca, who was present at the meeting, had a patient under his care at the time who had been aphasic (without power of speech) for twenty-one years, and who was in an almost moribund state. The autopsy proved of great interest, as it was found that the lesion was confined to the left side of the brain, and to what we now call the third frontal convolution. Broca was struck with the coincidence; and when a similar case came under his care afterwards, unaware of what had been done by Dax, he postulated the conclusion that the integrity of the third

frontal convolution, and perhaps also part of the second, is essential to speech. In a subsequent series of fifteen typical cases examined, it was found that the lesion had destroyed, among other parts, the posterior part of the third frontal in fourteen. In the fifteenth case the destruction had taken place in the island of Reil and the temporal lobe."

After 1861, physiology took the lead, and kept it. But, through all the work, science and practice have been held together; the facts of experimental physiology have been and are tested, every inch of the way, by the facts of medicine, surgery, and pathology. The infinite minuteness and complexity of the investigation, and its innumerable side-issues, are past all telling. They who are doing the work, in science and in practice, have always had in their thoughts the fear of fallacies in the interpretation of these highest forms of life. Sir William Gowers, fourteen years ago, wrote as follows of the earlier workers :—

"Doubt was formerly entertained as to the existence of differentiation of function in different parts of the cortex, but recent researches have established the existence of a differentiation which has almost revolutionised cerebral physiology, and has vastly extended the range of cerebral diagnosis. The first step of the new discovery was constituted by the clinical and pathological observations of Hughlings Jackson, which suggested the existence, on each side of the fissure of Rolando, of special centres for the movements of the leg, arm, and face. These observations led to the experiments of Ferrier, which resulted in the demonstration of the existence in the cortex of the lower animals of well-defined regions, stimulation of which caused separate movements, or evidence of special sense excitation, while the destruction of the same parts caused indications of a loss of the corresponding function.

Hence he came to the conclusion that these regions constitute actual motor and sensory centres. Ferrier had, however, been anticipated in many of these results by two German experimenters, Fritsch and Hitzig, whose results, differing a little in detail, correspond closely in their general significance. Many other investigations of the same character have since been made, of which those of Munk are especially important. The original observations of Hughlings Jackson left little doubt that the general facts, learned from experiments on animals, are true of man; and this conclusion has been to a large extent confirmed by pathological and clinical observations directed to the verification on man of the pathological results. To this verification the labours of Charcot and his coadjutors have largely contributed. But the verification has already made it probable that some differences exist between the brain of man and that of higher animals (even of monkeys), and that the conclusions from the latter cannot be simply transferred to the former."

Many and great difficulties, beyond this danger of the fallacy of "simple transference," beset every step of the work: it required the right use of the most delicate and susceptible instruments and tests, and the right understanding of anatomy, microscopic anatomy, comparative anatomy, organic chemistry, electricity, and physics: every moment of advance must be guarded, every word must be weighed. Among the earlier difficulties, was the failure of almost all the physiologists, before Hitzig, to produce muscular action by excitation of the cerebral cortex. Longet, Magendie, Flourens, Matteuci, Van Deen, Weber, Budge, and Schiff, had all failed. Hitzig (*Untersuchungen über das Gehirn*, Berlin, 1874) had observed, in man, that it was easy to produce movements of the eyes by the passage of the

constant current through the occipital region.[1] Taking this fact for a starting-point, he used a very low current, and thereby succeeded in producing certain definite muscular movements by stimulation of the cortex in animals. Of Hitzig's work, Sir Victor Horsley says :—

"It was not till 1870 that the next absolute proof (after Bell's work in 1813) was obtained of the localisation of function, so far as the highest centres of the nervous system were concerned. In that year Fritsch and Hitzig discovered that electrical excitation, with minimal stimuli, of various points of the cortex, caused those storehouses, of which Willis spoke, to discharge, and to reveal their function by the precise limitation of the groups of muscles which they were able to throw into action. These researches were abundantly confirmed and greatly extended by Professor Ferrier, and thus has been constructed in the history of this subject the most recent great platform or stage of permanent advance."[2]

The thirty years since Hitzig's work cannot be put here, for they would take a volume to themselves. There have been differences of interpretation of this or that fact, diversities of results, and problems too hard to solve, and other difficulties, such as befall all the natural sciences ; but these imperfections amount to very little, when the whole result comes to be reckoned. The marvel is that the work is so nearly perfect, seeing its immeasurable complexity.

[1] That the surface of the brain is not sensitive of such stimulation, that it does not perceive its own substance, was known to Aristotle. The fact is so familiar that there is no need to quote evidence of it, beyond that of Sir Charles Bell: "I have had my finger deep in the anterior lobes of the brain, when the patient, being at the time acutely sensible, and capable of expressing himself, complained only of the integument."

[2] Horsley, *Fullerian Lectures*, 1891, *loc. cit.*

Let any man, who has but touched the study of physiology, consider what is involved in even the most superficial observation of the simplest facts of the nervous system: for instance, the ordinary nerve-muscle preparation that is taught to every medical student, or the microscopic structure of the spinal cord, or the Wallerian method. Or let him consider how the physiology of the nervous system has been founded on the lower forms of life: the work of Romanes and others on the Medusa and the Echinodermata, and Huxley's work in biology, and the endless chain of forces that are alike in man and in jelly-fishes. Then let him try to estimate the output of hard thinking, for the advance from lower to higher structures, and up to man; the vigilant criticism of all theories and foregone conclusions, the incessant self-judgment and wearisome doubts and disputes all the way, elusiveness of facts, and vagueness of words. And the results thus wrung out of science had still to be stated in terms of practice, and tested by the facts of medicine, surgery, and pathology, and used in every hospital in the civilised world, not only for the saving of life, but also for the diagnosis and medical or surgical treatment of innumerable varieties of disease or injury of the brain, the cord, or the nerves. Sir Michael Foster, in a short summary of the problems of physiology, puts clearly these consummate difficulties of the physiology of the nervous system:—

"In the first place there are what may be called general problems, such as, How the food, after its preparation and elaboration into blood, is built up into the living substance of the several tissues? How the living substance breaks down into the dead waste? How the building up and breaking down differ in the different tissues in such a way that energy is set free

in different modes, the muscular tissue contracting, the nervous tissue thrilling with a nervous impulse, the secreting tissue doing chemical work, and the like? To these general questions the answers which we can at present give can hardly be called answers at all.

"In the second place there are what may be called special problems, such as, What are the various steps by which the blood is kept replenished with food and oxygen, and kept free from an accumulation of water; and how is the activity of the digestive, respiratory, and excretory organs, which effect this, regulated and adapted to the stress of circumstances? What are the details of the vascular mechanism by which each and every tissue is for ever bathed with fresh blood, and how is that working delicately adapted to all the varied changes of the body? And, *compared with which all other problems are insignificant and preparatory only*, how do nervous impulses so flit to and fro within the nervous system as to issue in the movements which make up what we sometimes call the life of man?"

The physiology of the nervous system is wrought to finer issues now than in the time of Bell and Magendie; and this generation of students may live to see the present facts and methods of cerebral localisation as the mere rudiments or elements of science. Happily for mankind, science has already so far elucidated them that they have done good service for the diagnosis and treatment of disease, and for the saving of lives.

Some examples have been given, in the foregoing chapters, of the value of physiological experiments on animals. It would be easy to lengthen the list, for there is no general subject in all physiology that does not owe something to this method: as Mr. Darwin

said, in his evidence before the Royal Commission of 1875, "I am fully convinced that physiology can progress only by the aid of experiments on living animals. I cannot think of any one step which has been made in physiology without that aid." Many examples have been left out altogether—the work of Boyle, Hunter, Lavoisier, Haldane, Despretz, and Regnault, on animal heat and on respiration; of Petit, Dupuy, Breschet, and Reid, on the sympathetic system; of Galvani, Volta, Haller, du Bois-Reymond, and Pflüger, on muscular contractility: nothing has been said of the work lately done on the suprarenal glands and "adrenalin," and on the blood-pressure in its relation to secretion. For the most part, only those examples have been taken that occur far back in the history of physiology: more has been said about the past than about the present. First, because it was necessary to put an end to the false statements that are made, by those who are opposed to all experiments on animals, about the work done in the past. Next, because the abstruse details of physiology, in the present, are not intelligible for general reading. Next, because it is impossible now to isolate physiology, or to say what belongs to physiology alone, to have back the simpler problems of the past, to discover the circulation of the blood twice. But the experimental method, alike in the past and in the present, has been the chief way of advance. And if a forecast may be made without offence, it is certain that the work of physiology, as in the past and the present, so in the near future, will exercise a profound influence for good on medical and surgical treatment. Among the subjects that especially occupy physiologists now are, the more exact localisation and interpretation of the special

sense-centres, and the better knowledge of the internal secretions and chemical influences of the glands and tissues of the body. It would be hard to find two fields of work more sure to favour the growth of the *arbor vitæ* side by side with the *arbor scientiæ*.

But the last word here must be said by a physiologist of the very highest authority, Professor Starling. He has kindly given me, for this edition, the following note:—

"Among the researches of the last thirty years, those bearing on the *Circulation of the Blood* must take an important place, both for their physiological interest and for the weighty influence they have exerted on our knowledge and treatment of disorders of the vascular system, such as heart disease. We have learned to measure accurately the work done by the great heart-pump; and by studying the manner in which this work is affected by different conditions, we are enabled to increase or diminish it, according to the needs of the organ. Experiments in what is often regarded as the most transcendental department of physiology—*i.e.* that which treats of muscle and nerve—have thrown light on the wonderful process of 'compensation,' by which a diseased heart is able to keep up a normal circulation.

"*Vaso-motor System.*—Largely by the labours of British physiologists, the exquisite control exercised by the nervous system over every blood-vessel in the body has been brought to light, the paths tracked out, and the mechanisms elucidated, by means of which the circulation through each part of the body is subordinated to the needs of the whole. Since the chief vaso-motor nerves take their course through the sympathetic system, the researches on their distribution have led to the mapping out of the whole of this system, and to an accurate knowledge of its functions. We are now acquainted with the course, to all parts of the body,

of the nerves which not only determine the changes in the calibre of the blood-vessels, but affect also the secretion of sweat and the erection of the hairs. Incidentally, the mapping out of these nerves, in the hands of Mackenzie, Head, and others, has led to more power of localising the seat of visceral disease.

"*Digestion.*—Our knowledge of the processes of digestion has of late years received a great accession by the work of Professor Pawlow, of St. Petersburg. His success is largely due to his recognition of the importance of keeping his experimental animals under the most normal conditions possible, and of studying the different parts of the alimentary tract in animals which were not anæsthetised, but which were free from any pain or even discomfort, either of which conditions materially interferes with the activity of the digestive glands. He therefore established in dogs fistulæ in chosen portions of the alimentary canal, analogous to the fistula which accident rendered so valuable in the case of Alexis St. Martin. Not only has the knowledge thus gained enabled the physician to understand the sequel of events in disordered digestion, but the success of the operative measures undertaken by physiologists for the elucidation of their science has emboldened surgeons to attack disease in the most various parts of the alimentary canal.

"Renewed study of the secretion of pancreatic juice evoked by the passage of the acid digestive products from the stomach into the small intestine, which had been described by Pawlow, has resulted in the discovery of a new class of chemical agents, which act as special messengers from one part of the body to another, and exercise an important function in determining the action of all parts to one common end.

"*Respiration.*—The investigation of the chemical properties of the colouring matter of blood, and of its compound with carbon monoxide, has resulted, in the hands of Dr. Haldane, in the laying down of measures for the prevention of accidents from choke-damp or

after-damp in mines. The same investigation has resulted in the discovery of a method of determining the total amount of blood circulating in the body of a living man. The application of this method has already added largely to our knowledge of the pathology of different forms of anæmia, as well as of the conditions obtaining in heart disease. Experiments by Hill and others on the physiological effects of compressed air have shown the precautions which should be observed in all diving operations. A proper appreciation of these results by diving-engineers would not only entirely obviate the cases of 'caisson disease,' but would enable diving to be carried on safely to a greater depth than has hitherto been attempted.

"It is impossible, however, to enumerate all the physiological gains of the last twenty or thirty years, or to point out their manifold applications in the cure and prevention of disease. The full control of the processes of disease, which is the goal of the physician and the surgeon, can only be attained through an accurate knowledge of the conditions governing the functions of the healthy body. The foundation of medicine and surgery is physiology: and it is only on living animals that the processes of life can be investigated."





PART II

EXPERIMENTS IN PATHOLOGY, MATERIA MEDICA, AND THERAPEUTICS



I

INFLAMMATION, SUPPURATION, AND BLOOD-POISONING

Pathology, the study of the causes and products of diseases, is a younger science than physiology: the use of the microscope was the beginning of pathology; and the microscope, even so late as sixty years ago, was very different to the microscope now. The great pathologists of that time had not the lenses, microtomes, and reagents that are now in daily employment; they knew nothing of the present methods of section-cutting and differential staining. But the publication in 1839 of Schwann's cell-theory marks the rise of modern pathology. In 1843, Darwin wrote his first draft of the doctrine of the origin of species; and Pasteur, that year, was in for his examination at the École Normale. The work of Schwann, Virchow, and Pasteur had such profound influences on science that the span of sixty years seems to cover the modern development of pathology: and this span of years is marked, half-way, by the rise of bacteriology. In 1875, when the Royal Commission on Experiments on Animals was held in London, the evidence was concerned practically with physiology alone: very little was said about pathology, and of bacteriology hardly a word. The witnesses say that they "believe they are beginning to get an idea" of the true nature of

tubercle: and the evidence as to the nature of anthrax, given by Sir John Simon, reads now like a very old prophecy:—

"We are going through a progressive work that has many stages, and are now getting more precise knowledge of the contagium. By these experiments on sheep it has been made quite clear that the contagium of sheep-pox is *something of which the habits can be studied, as the habits of a fern or a moss can be studied: and we look forward to opportunities of thus studying the contagium outside the body which it infects. This is not a thing to be done in a day, or perhaps in ten years, but must extend over a long period of time.* Dr. Klein's present paper represents one very important stage of a vast special study. He gives the identification of the contagium as *something which he has studied to the end in the infected body, and which can now in a future stage be studied outside the body.*"

Thirty years ago, there was no bacteriology, in the present sense of the word: and now the "habits" of these "contagia" have been studied, outside and inside the body, with amazing accuracy. It has been proved, past all possibility of doubt, that the pathogenic bacteria are the cause of infective diseases; they have fulfilled Koch's postulates—that they should be found in the diseased tissues, be cultivated outside the body, reproduce the same disease in animals, and be found again in the tissues of those animals. By an immeasurable amount of hard work crowded into a few years, this New World of bacteriology has been subdued. The Royal Commissioners of 1875, speaking of physiological experiments only, said, "It would require a voluminous treatise to exhibit in a consecutive statement the benefits that medicine and surgery

have derived from these discoveries." If physiology in 1875 required a treatise, bacteriology in 1906 requires a library: and it is impossible here to give more than the faintest outline of some of the work that has been done.

But all pathology is not bacteriology; and it would take a treatise of prodigious length to set forth the work of modern pathology in the years before anything was known of bacteria. The microscopic structure of tumours and of all forms of malignant disease, the nature of amyloid, fatty, and other degenerative changes, and the chief facts of general pathology—hypertrophy and atrophy, necrosis, gangrene, embolism, and many more—all these subjects were studied to good purpose, before bacteriology. Above all, men were occupied in the study of inflammation under the microscope. It was this use of the microscope that revolutionised pathology; especially, it made visible the whole process of inflammation, the most minute changes in the affected tissues, the slowing and arrest of the blood in the capillaries, the choking-up of the stream, and the escape of blood-cells out of the capillaries into the tissues. Everything had been made ready for the fuller interpretation that was coming from bacteriology: the old naked-eye descriptions of inflammation were left behind; men set aside the definition of Celsus, that it was *rubor et tumor cum calore et dolore*—words that sound like Molière's jest about the *vis dormitiva* of opium—they watched inflammation under the microscope, in such transparent structures as the frog's web and mesentery, the bat's wing, and the tadpole's tail. It was thus that Wharton Jones discovered the rhythmical contraction of the veins in the bat's wing. The discovery of the escape

of the white blood-cells, *diapedesis*, through the walls of the capillaries, was made by Waller and Cohnheim. To those who are opposed to all experiments on animals, it may seem a very small thing that a blood-cell should be on one side or the other of a microscopic film in a tadpole's tail; but this *diapedesis*, the first move of the blood in its fight against disease, is now seen, in the light of Metschnikoff's work, as a fact of very great importance.

The history of this transitional period, from the study of inflammation in transparent living tissues to the use, in surgery, of the facts of bacteriology, is told in Lord Lister's Huxley Lecture, October 1900. He describes how the foundations were laid in surgical pathology, by microscopical and experimental work on inflammation, coagulation, suppuration, and pyæmia, for bacteriology to build on: how his own share of the work began when he was house-surgeon to Sir John Erichsen at University College Hospital, and afterward to Mr. Syme in Edinburgh, and how it was continued through all his Edinburgh and Glasgow life:—

"After being appointed to the Chair of Surgery in the University of Glasgow, I became one of the surgeons to the Royal Infirmary of that city. Here I had, too, ample opportunities for studying hospital diseases, of which the most fearful was pyæmia. About this time I saw the opinion expressed by a high authority in pathology that the pus in a pyæmic vein was probably a collection of leucocytes. Facts such as those which I mentioned as having aroused my interest in my student days in a case of pyæmia, made such a view to me incredible; and I determined to ascertain, if possible, the real state of things by experiment. . . .

"While these investigations into the nature of pyæmia were proceeding, I was doing my utmost against that

deadly scourge. Professor Polli, of Milan, having recommended the internal administration of sulphite of potash on account of its antiputrescent properties, I gave that drug a very full trial as a prophylactic. . . . At the same time, I did my best, by local measures, to diminish the risk of communicating contagion from one wound to another. I freely employed antiseptic washes, and I had on the tables of my wards piles of clean towels to be used for drying my hands and those of my assistants after washing them, as I insisted should invariably be done in passing from one dressing to another. But all my efforts proved abortive; as I could hardly wonder when I believed, with chemists generally, that putrefaction was caused by the oxygen of the air.

"It will thus be seen that I was prepared to welcome Pasteur's demonstration that putrefaction, like other true fermentations, is caused by microbes growing in the putrescible substance. Thus was presented a new problem: not to exclude oxygen from the wounds, which was impossible, but to protect them from the living causes of decomposition by means which should act with as little disturbance of the tissues as is consistent with the attainment of the essential object. . . . To apply that principle, so as to ensure the greatest safety with the least attendant disadvantage, has been my chief life-work.[1]

And, of course, the application of that principle is not limited to the performance of the major operations of surgery. It is in daily use in every hospital, and in every practice all the world over, for the safe and quick healing of whole legions of injuries, "casualties," and minor operations.

But what of Semmelweis, and his study of puerperal fever? Did he not, before Lord Lister, and without the help of experiments on animals, discover antiseptic

[1] See also the admirable Life of Pasteur, by M. Valléry-Radot. Translation by Mrs. Devonshire, vol. ii. p. 20.

surgery? His claim is urged by those who are opposed to all such experiments. And the answer is, that his work was lost just for want of experiments on animals. If he could have demonstrated, as Pasteur did, the living organism, the thing itself, there in the tissues of an infected rabbit, and in a test-tube, and under a microscope, he might have stopped the mouths of his adversaries. He could not. He could only demonstrate to them the fact that their patients died, and his patients lived: and that some sort of direct infection was the cause of the deaths. The tragedy of his life cannot be told too often, and may be told again here.[1] For want of the final proof that bacteriology, and the inoculation of animals, alone could give, he was unable to hold out against his enemies till Pasteur could rescue him.

In 1846, when he was twenty-three years old, Ignaz Semmelweis was appointed assistant-professor in the maternity department of the huge general hospital of Vienna. For many years, the mortality in the lying-in wards had been about 1.25 per cent., and no more. Then, under a new professor, it had risen; and, for some years before Semmelweis came on the scene, it had been 5 per cent., or even 7 per cent. In October 1841, there had been an epidemic that had lasted till May 1843. In these twenty months, out of 5139 women delivered, 829 had died; that is to say, 16 per cent.

There were two sets of wards in the maternity department. The one set may be called Clinique A, and the other Clinique B. For many years, the mortality had been the same in each. In 1841 a change was

[1] This account of Semmelweis, reprinted by permission from the *Middlesex Hospital Journal*, is mostly taken from Dr. Theodore Duka's excellent paper on "Childbed Fever." (*Lancet*, 1886.)

made: Clinique A was assigned to the teaching of students, and Clinique B to the teaching of midwives: and, so soon as this change had been made, the mortality in Clinique B became less, but the mortality in Clinique A did not. Commissions of inquiry were held, and in vain. It was suggested that the foreign students were somehow to blame, nobody knew why; and many of them were sent away. Still the deaths went on. Women admitted to Clinique A would go down on their knees and pray to be allowed to go home; almost every day the bell was heard ringing in the wards, for the administration of the Sacrament to a dying woman. People talked about atmospheric influences, and overcrowding, and the tainted air of old wards, and the power of the mind over the body: and Semmelweis set to work.

He observed that cases of protracted labour in Clinique A died, almost all of them; but not in Clinique B. He observed also that cases of premature labour, nearly all of them, did well, whichever Clinique they were in; so did those women who were delivered before they came to the hospital, and were admitted after delivery. He observed also that a row of patients, lying side by side, would all be attacked at once in Clinique A; which never happened in Clinique B. He tried everything: he altered the details of treatment; he used various subterfuges to prevent one of the professors from examining serious cases; he enforced this or that rule in Clinique A, because it was the custom in Clinique B; he slaved away at the notes of the cases—and at last the truth came to him, by the death of one of his friends from a dissection-wound. He says, "My friend's fatal symptoms unveiled to my mind an identity with those which I had so often noticed at the deathbeds of puerperal cases." He saw now that the

students, coming straight from the dissecting-rooms, had infected the patients during examination.

In May 1847 he gave orders that every student, before examining, should thoroughly disinfect his hands. But, though he had reckoned with dissecting-room poisons, he had forgotten to reckon with other sources of infection. In October of that year, a woman was admitted who had malignant disease; of twelve women examined after her, eleven got puerperal fever, and died. In November, a woman was admitted who had a suppurating knee-joint, with sinuses; and eight women were infected from her, and died. Therefore Semmelweis said, "Not only can the particles from dead bodies generate puerperal fever, but any decomposed material from the living body can also generate it, and so can air contaminated by such materials." Henceforth he isolated all infected cases, he enforced the strict use of disinfectants: and the mortality in Clinique A, which in May 1847 had stood at 12.24 per cent., fell in December to 3.04, and in 1848 was 1.27.

His work was taken up with enthusiasm by Hebra, Skoda, and Haller; the news of it was sent to every capital in Europe. In February 1849 Haller read a paper on it before the Medical Society of Vienna, and said, "The importance of these observations is above all calculation, *both for the maternity department and for the hospitals in general, but particularly for the surgical wards.*" A committee was nominated to report on the whole matter; but it was opposed by the professor in charge of Clinique A, and nothing came of it. In May 1850, Semmelweis opened a great debate on puerperal fever, which occupied three sittings of the Vienna Medical Society. His opponents were there in full force, all the Scribes and Pharisees of the profession. They brought

about a vague distrust of his figures and his facts; they got people to believe that there must be "something else" in puerperal fever, as well as the local infection. Semmelweis began to be discouraged. The University authorities made a dead set against him—they refused to renew his appointment, they got him out of the hospital, and out of Vienna. He went to Pesth, and was Professor of Midwifery there; but the same opposition and hostility were at Pesth as at Vienna. Slowly he began to lose his hold over himself, went down hill, became excitable and odd. The end came in July 1865. At a meeting of University professors, he suddenly took a paper from his pocket and read aloud to them a solemn oath, to be enforced on every midwife and every doctor. His mind had given way: he was moved to an asylum at Vienna, and died there a few weeks later. He was only forty-two when he died—*What a wounded name, Things standing thus unknown, shall live behind me.*

The contrast between the work of Semmelweis and the work of Pasteur cuts like a knife here. The failure of Semmelweis' teaching may be estimated by the fact that it had all to be done over again. The year of his success at Vienna was 1848. Eight years later, in the Paris Maternity Hospital, between 1st April and 10th May 1856, came such an outbreak of puerperal fever that out of 347 patients 64 died. In 1864, out of 1350 cases, 310 deaths. In Jan.–Feb. 1866, out of 103 cases, 28 deaths: "Women of the lower classes looked upon the Maternité as the vestibule of death." In 1877–78, came the use of carbolic acid and perchloride of mercury at the hospital, thirty years after Semmelweis' work: and, about the same time, Pasteur's discovery of the streptococcus in puerperal fever.[1] Pasteur

[1] See Pasteur's Life, vol. ii. p. 89.

could demonstrate to his opponents the visible cause of the infection, the thing itself. Roux tells the story :—

"Dans le pus des abcès chauds et dans celui des furoncles on constate un petit organisme arrondi, disposé en amas, qu'on cultive facilement dans le bouillon. On le retrouve dans l'ostéomyélite infectieuse des enfants. Pasteur affirme que l'ostéomyélite et le furoncle sont deux formes d'une même maladie, et que l'ostéomyélite est le furoncle de l'os. En 1878, cette assertion a fait rire bien les chirurgiens.

"Dans les infections puerpérales, les caillots renferment un microbe à grains arrondis se disposant en files. Cet aspect en chapelet est surtout manifesté dans les cultures. Pasteur n'hésite pas à déclarer que cet organisme microscopique est la cause la plus fréquente des infections chez les femmes accouchées. Un jour, dans une discussion sur la fièvre puerpérale à l'Académie de Médicine, un de ses collégues le plus écoutés dissertait éloquemment sur les causes des épidémies dans les maternités. Pasteur l'interrompt de sa place: *Ce qui cause l'épidémie, ce n'est rien de tout cela: c'est le médecin et son personnel qui transportent le microbe d'une femme malade à une femme saine.* Et comme l'orateur répondit qu'il craignait fort qu'on ne trouve jamais ce microbe, Pasteur s'élance vers le tableau noir, dessine l'organisme en chapelet de grains, en disant, *Tenez, voici sa figure.*" (Roux, *L'Œuvre Médicale de Pasteur. Agenda du Chimiste,* 1896, p. 528.)

All suppuration, and all forms of "blood-poisoning" —abscesses, boils, carbuncles, erysipelas, puerperal fever, septicæmia, pyæmia—are due to minute organisms, various kinds of *micrococcus.* It has indeed been shown that suppuration may, in exceptional conditions, occur without micro-organisms: but practically every case of suppuration is a case of infection either from without or from within the body. There

is no room here for any account of the work spent on these micrococci: on their identification, isolation, culture, and inoculation. It is the same with all the pathogenic bacteria—each kind has its own habits, phases and idiosyncrasies, antagonisms and preferences: nothing is left unstudied—the influences of air, light, heat, and chemistry; all the facts of their growth, division, range of variation, grades of virulence, vitality, and products; the entire life and death of each species, and everything that it is, and does, and can be made to do. The difficulties of bacteriology are written across every page of the text-books: above all, the difficulties of attenuating or intensifying the virulence of bacteria, and of immunising animals, and of procuring from them an immunising serum of exact and constant strength. Every antitoxin is the outcome of an immeasurable expenditure of hard international work, unsurpassed in all science for the fineness of its methods and the closeness of its arguments.

The older theories of disease had attributed infection to the intemperature of the weather, the powers of the air, or the work of the devil; later, men recognised that there must be a *materies morbi*, something particulate, transmissible, and perhaps alive, but it was still a "nameless something." Therefore, they over-estimated the constitutional, personal aspect of a case of infective disease, against the plain evidence of case-to-case infection or inoculation: they studied with infinite care and minuteness the weather, the environment, the family history, the previous illnesses of the patient—everything, except the immediate cause of the trouble. But modern pathology, like Pasteur, says, *Tenez, voici sa figure.*

The antiseptic method was based on bacteriology,

resting as it did on the proof afforded by Pasteur that putrefaction was caused by bacteria, and not by the oxygen of the air, as had been previously believed. If any man would measure one very small part of the lives that are saved by this method, let him contrast the treatment of empyema fifty years ago with its treatment now. If he would measure the saving, not of lives but of limbs, let him take the treatment of compound fractures. If he would measure the saving of patients from pain, fever, and long confinement to bed, let him take the ordinary run of surgical cases, not only the major operations but all abscesses, lacerated wounds, foul sores, and so forth.

A serum has also been used of late years for the treatment of micrococcus-infection, and has given good results in many cases. It has been used, also, to avert the risk of such infection in certain operations where the antiseptic method cannot be strictly carried out. For the use of a "polyvalent" serum, reference may be made to the recent paper by Dr. W. S. Fenwick and Dr. Parkinson. (*Trans. Roy. Med. Chir. Soc.*, 1906.)

II

ANTHRAX

In animals, anthrax is also called *charbon*, splenic fever, or splenic apoplexy: in man, the name of *malignant pustule* is given to the sore at the point of accidental inoculation, and the name of *woolsorter's disease* is given to those cases of anthrax where the lungs are infected by inhalation of the spores of the *bacillus anthracis*. The disease occurs among hide-dressers, woolsorters, brushmakers, and rag-pickers: among animals, it occurs in sheep, cattle, horses, and swine:—

"Many of the outbreaks of anthrax in England have been in the neighbourhood of Bradford, and have been traced to the use of infected wool-refuse as manure. A map published by the Board of Agriculture shows that the outbreaks of anthrax are most frequent in those counties of Great Britain where dry foreign wools, hairs, hides, and skins are manufactured into goods. In 1892, there were forty-two outbreaks of anthrax in the West Riding of Yorkshire, as against two in the North Riding, and one in the East Riding. An undoubted fact in connection with anthrax is its tendency to recur on certain farms. During 1895, the disease reappeared on twenty-three farms or other premises in England, and six in Scotland, where it had been reported in the previous year." (Dr. Poore's Milroy Lectures, *On the Earth in relation to Contagia*, 1899.)

An admirable account of the disease, as it occurs in man, is given by Dr. Hamer and Dr. Bell, in the valuable series of monographs edited by Dr. Oliver of Newcastle, under the title *Dangerous Trades* (London, John Murray, 1902). Happily, the disease is very rare among men, even among those most exposed to it. For its treatment in man, an antitoxin has been used with some success: but the cases are too few to be of much importance.[1]

The *bacillus anthracis* was first seen more than fifty years ago: "Anthrax has the distinction of being the first infectious disease the bacterial nature of which was definitely proven."[2] Pollender in 1844, Roger and Davaine in 1850, noted the *petits bâtonnets* in the blood of sheep dead of the disease, and thought they were some sort of microscopic blood-crystals: it was not till 1863, after Pasteur's study of lactic-acid fermentation, that Davaine realised they were living organisms. Afterward, Koch succeeded in making cultures of them, and reproduced the disease by inoculating animals with these cultures; yet it was said, so late as 1876, that the *bacillus anthracis* was not the cause of anthrax, but only the sign of it: "Along with the bacilli, there are blood-cells and blood-plasma, and these contain the true amorphous virus of anthrax." Then came Pasteur's work, and reached its end in the experiments at Chartres, and the famous test-inoculations (1881) at Pouilly-le-Fort.

[1] Dr. Legge, in his Milroy Lectures, 1905, on Industrial Anthrax (*Lancet*, March and April 1905), gives a full account of Sobernheim's work up to March 1904, and a table of seventy-six cases, treated with Sclavo's serum.

[2] See Dr. Flexner's account of the disease, in volume xix. of Stedman's *Twentieth Century Practice*.

In the *Agenda du Chimiste* (1896) M. Roux gives the following account of this work, which he watched from first to last:—

"Vaccination against *charbon* has now been put to the test of practice for fourteen years. Wherever it is adopted, there the losses from *charbon* have become insignificant. It was followed by vaccination against swine-measles, *rouget des porcs*, the special study of our poor friend Thuillier. But the immediate result of Pasteur's vaccinations is their least merit: they have given men absolute faith in a science that could show such good works, they have started a movement that is irresistible; above all, they have set going the whole study of immunity, which is bringing us at last to a right way of treating infective diseases.

"Virulence is a quality that microbes can lose, or can acquire. Suppose we came across the anthrax-bacillus so far attenuated, in the way of Nature, that it had lost all power to kill—of course we should fail to recognise it; we should take it for an ordinary bacillus of putrefaction: you must watch it through each phase of its attenuation, to know that the harmless organism is the descendant of the fatal virus. But you can give back to it the virulence that it has lost, if you put it, to begin with, under the skin of a very delicate subject, a mouse only one day old. With the blood of this mouse inoculate another, a little older, and it will die. Passing by this method from younger to older mice, we come to kill adult mice, guinea-pigs, then rabbits, then sheep, etc. Thus, by transmission, the virus gains strength as it goes. Doubtless this increase of virulence, that we bring about by experiment, occurs also in Nature; and it is easy to see how a microbe, usually harmless to this or that species of animals, might become deadly to it. Is not this the way that infective diseases have appeared on the earth from age to age?

"*See how far we have come, from the old metaphysical*

ideas about virulence, to these microbes that we can turn this way or that way—stuff so plastic that a man can work on it, and fashion it as he likes."

Pasteur's note on the attenuation of anthrax was presented to the Académie des Sciences on 28th February 1881 ; and the test-inoculations at Pouilly-le-Fort were made in May of that year. It was hardly to be expected that every country, in every year, should obtain such results as France now takes as a matter of course ; and at one time, about twenty-one years ago, there was in Hungary a "conscientious objection" to the inoculation of herds against the disease. But in Italy, from 1st May 1897 to 30th April 1898, the issue of anti-charbon vaccine from one institute alone, the Sero-Therapeutic Institute at Milan, was 165,000 tubes, enough to inoculate 33,734 cattle and 98,792 sheep. And in France, between 1882 and 1893, more than three million sheep, and nearly half a million cattle, were inoculated.

The work done in France was published by M. Chamberland, in the *Annales de L'Institut Pasteur*, March 1894. The following translation of his memoir —*Résultats pratiques des Vaccinations contre le Charbon et le Rouget en France*—shows something of the national influence of the Pasteur Institute:—

1. *Charbon*

"After the famous experiments at Pouilly-le-Fort, MM. Pasteur and Roux entrusted to me the whole method and practice of the vaccinations against *charbon*. Twelve years have passed, and it is now time to put

together the results, and to make a final estimate of the value of these preventive inoculations.

"Every year we ask the veterinary surgeons to report—

1. The number of animals they have vaccinated.
2. The number that have died after the first vaccination.
3. The number that have died after the second vaccination, within the twelve days following it.
4. The number that have died during the rest of the year.
5. The average annual mortality before the practice of vaccination.

"The sum total of all the reports is given in the following tables:—

VACCINATION AGAINST CHARBON (FRANCE).

Sheep.

Years.	Total Number of Animals Vaccinated.	Number of Reports.	Animals Vaccinated according to Reports received.	Mortality.			Total.	Total loss per 100.	Average loss before Vaccination.
				After First Vaccination.	After Second Vaccination.	During the rest of the Year.			
1882	270,040	112	243,199	756	847	1,037	2,640	1.08	10%
1883	268,505	103	193,119	436	272	784	1,492	0.77	,,
1884	316,553	109	231,693	770	444	1,033	2,247	0.97	,,
1885	342,040	144	280,107	884	735	990	2,609	0.93	,,
1886	313,288	88	202,064	652	303	514	1,469	0.72	,,
1887	293,572	107	187,811	718	737	968	2,423	1.29	,,
1888	269,574	50	101,834	149	181	300	630	0.62	,,
1889	239,974	43	88,483	238	285	501	1,024	1.16	,,
1890	223,611	69	69,865	331	261	244	836	1.20	,,
1891	218,629	65	53,640	181	102	77	360	0.67	,,
1892	259,696	70	63,125	319	183	126	628	0.99	,,
1893	281,333	30	73,939	234	56	224	514	0.69	,,
Total	3,296,815	990	1,788,879	5,668	4,406	6,798	16,872	0.94	10%

VACCINATION AGAINST CHARBON (FRANCE).

Cattle.

Years.	Total Number of Animals Vaccinated.	Number of Reports.	Animals Vaccinated according to Reports received.	Mortality.			Total.	Total loss per 100.	Average loss before Vaccination.
				After First Vaccination.	After Second Vaccination.	During the rest of the Year.			
1882	35,654	127	22,916	22	12	48	82	0.35	5%
1883	26,453	130	20,501	17	1	46	64	0.31	,,
1884	33,900	139	22,616	20	13	52	85	0.37	,,
1885	34,000	192	21,073	32	8	67	107	0.50	,,
1886	39,154	135	22,113	18	7	39	64	0.29	,,
1887	48,484	148	28,083	23	18	68	109	0.39	,,
1888	34,464	61	10,920	8	4	35	47	0.43	,,
1889	32,251	68	11,610	14	7	31	52	0.45	,,
1890	33,965	71	11,057	5	4	14	23	0.21	,,
1891	40,736	68	10,476	6	4	4	14	0.13	,,
1892	41,609	71	9,757	8	3	15	26	0.26	,,
1893	38,154	45	9,840	4	1	13	18	0.18	,,
Total	438,824	1,255	200,962	177	82	432	691	0.34	5%

"Comparing the figures in the fourth column with those in the second, we see that a certain number of veterinary surgeons neglect to send their reports at the end of the year. The number of reports that come to us even tends to get less each year. The fact is, that many veterinary surgeons who do vaccinations every year content themselves with writing, 'The results are always very good; it is useless to send you reports that are always the same.'

"We have every reason to believe, as a matter of fact, that those who send no reports are satisfied; for if anything goes wrong with the herds, they do not fail to let us know it at once by special letters.

"Anyhow, thanks chiefly to new veterinary surgeons who do send reports, we see that in the twelve years,

up to 1st January of this year, we have had exact returns as to 1,788,879 sheep and 200,962 cattle—about half of all those that were vaccinated.

"The mortality among sheep and cattle is slightly higher after the first vaccination than after the second. This fact seems to us easy to explain. The animals reported dead include both those that died as the result of the vaccinations, and those that, being already infected at the time, died of the actual disease. But, at the time of second vaccination, the animals are already more or less protected: hence a lower mortality from the actual disease, and a lower sum total.

"The whole loss of sheep is about 1 per cent.: the average for the twelve years is 0.94. So we may say that *the whole average loss of vaccinated sheep, whether from vaccination or from the disease itself, is about* 1 *per cent.* The loss of vaccinated cattle is still less: for the period of twelve years, it is 0.34, or about $\frac{1}{3}$ per cent.

"These results are extremely satisfactory. It is to be noted especially that the average annual death-rate from *charbon*, before vaccination—the average given in these reports—is estimated at 10 per cent. among sheep, and 5 per cent. among cattle. But even if we put it at 6 per cent. for sheep, and $3\frac{1}{3}$ per cent. for cattle, and say that the worth of a sheep is 30 francs, and of an ox or a cow 150 francs—which is well below their real value—even then it is obvious that the advantage of these vaccinations to French agriculture is about five million francs in sheep, and two million in cattle. And these figures are rather too low than too high.

2. *Rouget*

"Some years after the discovery of vaccination against *charbon*, M. Pasteur discovered the vaccine for a disease of swine known under the name of ***rouget.*** From 1886, these vaccines were prepared and sent out under the same conditions as the vaccines against *charbon.* The following table gives the reports that have come to us of this disease:[1]—

VACCINATION AGAINST ROUGET (FRANCE).

Years.	Total Number of Animals Vaccinated.	Number of Reports.	Animals Vaccinated according to Reports received.	Mortality.			Total.	Total loss per 100.	Average loss before Vaccination.
				After First Vaccination.	After Second Vac	During the rest of the Year.			
1886	For these two years France and other countries are put together.	49	7,087	91	24	56	171	2.41	20%
1887		49	7,467	57	10	23	90	1.21	,,
1888	15,958	31	6,968	31	25	38	94	1.35	,,
1889	19,338	41	11,257	92	12	40	144	1.28	,,
1890	17,658	41	14,992	118	64	72	254	1.70	,,
1891	20,583	47	17,556	102	34	70	206	1.17	,,
1892	37,900	38	10,128	43	19	46	108	1.07	,,
Total	111,437	296	75,455	534	188	345	1,067	1.45	20%

"*The total average of losses during the past seven years is* 1.45 *per cent., or about* 1½ *per cent.*

"This average is appreciably higher than the average for *charbon.* But it must be noted that the mortality

[1] "The reports for 1893 are at present too few to be utilised for this table."

from *rouget* among swine, before vaccination, was much higher than that from *charbon* among sheep. It was about 20 per cent.; a certain number of reports speak of losses of 60 and even 80 per cent.: so that almost all the veterinary surgeons are loud in their praises of the new vaccination."

The rest of M. Chamberland's paper is concerned with the defects, such as they are, of the vaccinations, and the need of absolute cleanliness in the making of them: which is somewhat difficult for this vast number of vaccinations of animals all over France, and in other parts of the world. The whole story of the discovery is told in M. Valléry-Radot's Life of Pasteur: and the whole story of *rouget*, in the same most fascinating book, vol. ii., p. 180.

III

TUBERCLE

Before Laennec, tubercle had been taken for a degenerative change of the tissues, much like other forms of degeneration. It was Laennec who brought men to see that it is a disease of itself, different from anything else; and this great discovery of the specific nature of tubercle, and his invention of the stethoscope, place him almost level with Harvey. He founded the facts of tubercle, and on that foundation Villemin built. In 1865, Villemin communicated to the Académie des Sciences his discovery that tubercle is an infective disease; that he had produced it in rabbits, by inoculating them with tuberculous matter. *En voici les preuves*, he said. He appealed to these inoculations to prove his teaching :—

La tuberculose est une affection spécifique.
Sa cause réside dans un agent inoculable.
L'inoculation se fait très-bien de l'homme au lapin.
La tuberculose appartient donc à la classe des maladies virulentes.

It was no new thing to say, or to guess, that phthisis was or might be infective. So far back as 1500, Frascatorius had said that phthisis came "by the gliding of the corrupt and noisome humours of the patient into the lungs of a healthy man." Surely, if clinical

experience could suffice, men would have made something out of this wisdom of Frascatorius. They made nothing of it; they waited three hundred years for Villemin to inoculate the rabbits, and then the thing was done—*En voici les preuves.* Three years later, Chauveau produced the disease in animals, not by inoculation, but by the admixture of tuberculous matter with their food. Then, as the work grew, there came a short period of uncertainty: different species of animals are so widely different in their susceptibility to the disease that the results of further inoculations seemed to go against Villemin; and it was not till 1880 that Cohnheim finally established Villemin's teaching, and even went beyond it, making inoculation the very proof of tubercle:—

"Everything is tuberculous, that can produce tuberculous disease by inoculation in animals that are susceptible to that disease: and nothing is tuberculous, that cannot do this."

Then, in 1881, came the welcome news that Koch had discovered the bacillus of tubercle. In his first published account of it (24th March 1882) he says:—

"Henceforth, in our warfare against this fearful scourge of our race, we have to reckon not with a nameless something, but with a definite parasite, whose conditions of life are for the most part already known, and can be further studied. . . . Before all things, we must shut off the sources of the infection, so far as it is in the power of man to do this."[1]

[1] "In Zukunft wird man es im Kampf gegen diese schreckliche Plage des Menschengeschlechtes nicht mehr mit einem unbestimmten Etwas, sondern mit einem fassbaren Parasiten zu thun haben, dessen Lebensbedingungen zum grössten Theil bekannt sind und noch weiter erforscht werden. Es müssen vor allen Dingen die Quellen, aus denen der Infections-stoff fliesst, so weit es in menschlichen Macht liegt, verschlossen werden."

In November 1890 he announced, in the *Deutsche Medizinische Wochenschrift*, the discovery of tuberculin. Its failure was one of the world's tragedies. The defeat may not be final, and we may live to see phthisis fought and beaten with its own weapons: but, for the present, it is more to the purpose to consider what other benefits have been gained, from the discovery of the tubercle-bacillus in 1881, in every civilised country in the world.

1. It has given to everybody a more reasonable and hopeful view of phthisis and the diseases allied to it. The older doctrine of heredity, that the child inherits the disease itself, has given way to the doctrine that the inheritance, in the vast majority of cases, is not that of the disease itself, but that of a tendency or increased susceptibility to the disease.

2. It has brought about an immense improvement in the early and accurate diagnosis of all cases. The bacillus found in the sputa, or in the discharges, or in a particle of tissue, is evidence that the case is tuberculous.

3. It has given evidence, which till 1901 was hardly called in question,[1] that *tabes mesenterica*, a tuberculous disease which kills thousands of children every year, is due in many cases to infection from the milk of tuberculous cows. In England alone, in 1895, the number of children who died of this disease was 7389, of whom 3855 were under one year old.

4. It has proved, and has taught everybody to see

[1] At the British Congress on Tuberculosis, London, 1901, Koch stated that bovine tuberculosis and human tuberculosis are not one and the same disease, and that the risk of milk-infection is so small that burdensome restrictions ought not to be enforced. In the general judgment of men well qualified to study the subject, he failed to prove his point.

the proof, that the sputa of phthisical patients are the chief cause of the dissemination of the disease. By insisting on this fact, it has profoundly influenced the nursing and the home-care of phthisical patients; and it has begun to influence public opinion in favour of some sort of notification of the disease, and in favour of enforcing a law against spitting in public places and conveyances. In some of the principal cities of the United States, laws on this subject have already been enacted.

5. It has greatly helped to bring about the present rigorous control of the meat and milk trades. The following paragraph, taken almost at random, will suffice here:—

"Bacteriological examinations during the past year have shown that more milks are tuberculosis-infected than is generally supposed, and the importance of carefully supervising milk supplies is becoming more and more acknowledged. Veterinary surgeons are practically agreed that tuberculin is a reliable and safe test for diagnosing the presence of tuberculosis in animals, but affords no index of the extent or degree of the disease. The test, however, will not produce tuberculosis in healthy animals, and has no deleterious effect upon the general health of the animals. The London County Council have decided that all cows in London cowsheds shall be inspected by a veterinary surgeon regularly once in every three months, and that a systematic bacteriological examination shall be conducted of milks collected from purveyors." (*Medical Annual*, 1901.)

6. Tuberculin has come into general use for the detection of tuberculosis in cattle, to "shut off the sources of the infection." A full account of this method in different countries was given by Professor Bang, of Copenhagen, at the Fourth Congress on Tuberculosis,

Paris, 1898. The injection of tuberculin is followed in eight to twelve hours by a well-marked rise of temperature, if the animal be tuberculous. Of this test, Professor McFadyean, Principal of the Royal Veterinary College, London, says:—

"I have no hesitation in saying that, taking full account of its imperfection, tuberculin is the most valuable means of diagnosis in tuberculosis that we possess. . . . I have most implicit faith in it, when it is used on animals standing in their own premises and undisturbed. It is not reliable when used on animals in a market or slaughter-house. A considerable number of errors at first were found when I examined animals in slaughter-houses after they had been conveyed there by rail, etc. Since that, using it on animals in their own premises, I have found that it is practically infallible. I have notes of one particular case, where twenty-five animals in one dairy were tested, and afterwards all were killed. There was only one animal which did not react, and it was the only animal not found to be tuberculous when killed."

Two instances of the validity of this test will suffice. In 1899, it was applied to 270 cows on some farms in Lancashire. Of these cows, 180 reacted to the test, 85 did not react, and 5 were doubtful. Tuberculous disease was actually found, when they were killed, in 175 out of the 180=97.2 per cent. (*Lancet*, 5th August 1899.) In 1901, Arloing and Courmont published a critical account of the whole subject, and gave the following facts. In 80 calves, which on examination after death were found not tuberculous, the test was negative: in 70 older cattle, which were tuberculous, the test was positive in every case but one,

though the dilution of the serum was 1 in 10.[1] It would be easy to add instances of the value of this test, for it is practised far and wide over the world.

7. More recently, the discovery of the "opsonic index," and its use by Sir Almroth Wright and others, has given a great advance to the observation and treatment of cases of tuberculosis. The administration of the "new tuberculin" is now timed and measured with an accuracy which was absolutely impossible a few years ago.

It is a far cry, from the present method of counting how many tubercle-bacilli are taken up by a single blood-cell, back to Villemin's rabbits. Every inch of the way, from 1881 onward, the pathological study of every form of tuberculosis, medical or surgical, human or bovine, has been dependent on bacteriology; that is to say, on experiments on animals.

[1] For references to this paper, and to evidence put forward against the validity of the test, and for criticism of such evidence, see Gould's *Year-Book of Medicine and Surgery*, 1902 (Philadelphia, W. B. Saunders & Company).

IV

DIPHTHERIA

THE bacillus of diphtheria, the Klebs-Loeffler bacillus, was first described by Klebs in 1875, and was first obtained in pure culture by Loeffler in 1884. Its isolation was a matter of great difficulty, and the work of many years, because of its association in the mouth with other species of bacteria. The following table, from Hewlett's *Manual of Bacteriology*, is a good instance of one of many practical difficulties. Out of 353 cases of diphtheria, bacteriological examination found the diphtheria-bacillus alone in 216 cases. In the remaining 137 it was associated with the following organisms:—

Streptococci	6
Staphylococci	55
Bacilli	19
Torulæ	9
Sarcinæ	6
Streptococci and micrococci	2
Micrococci and bacilli	9
Streptococci and bacilli	1
Torulæ and bacilli	1
Micrococci and sarcinæ	6
Micrococci and torulæ	4
Many forms present together	19
	137

In December 1890 came the news that Behring and Kitasato had at last cleared the way for the use of an antitoxin:—

"Our researches on diphtheria and on tetanus have led us to the question of immunity and cure of these two diseases; and we succeeded in curing infected animals, and in immunising healthy animals, so that they have become incapable of contracting diphtheria or tetanus."

Aronsen, Sidney Martin, Escherich, Klemensiewicz, and many more, were working on the same lines; and in 1893, Behring and Kossel and Heubner published the first cases treated with antitoxin. Then, in 1894, came the Congress of Hygiene and Demography at Budapest, and Roux's triumphant account of the good results already obtained. Thus the treatment is not many years old; but, if the whole world could tabulate its results, the total number of lives saved would already be somewhere above a quarter of a million. Men found it hard at first to believe the full wonder of the discovery: the medical journals of 1895 and 1896 still contain the fossils of criticism — all the *may be* and *must be* of the earlier debates on the new treatment. The finest of all these fossils is embedded in the *Saturday Review* of 2nd Feb. 1895—*It is a pity that the English Press should continue to be made the cat's-paw of a gang of foreign medical adventurers.* To get at the truth, we must reckon in thousands: take, out of a whole mass of evidence, all just alike, the reports from London, Berlin, Munich, Vienna, Strasbourg, Cairo, Boston, and New York; these to begin with. Or the following facts, cut almost at random out of the medical journals:—

"The medical report of the French army states that since the introduction of the serum-treatment of diphtheria, the mortality among cases of that disease had fallen from 11 per cent. to 6 per cent." (*Brit. Med. Journ.*, 3rd September 1898.)

"Professor Krönlein (Zürich) exhibited statistical tables, showing that the prevalence of diphtheria in the canton of Zürich had been nearly uniform during the past fifteen years; and that the mortality rapidly decreased as soon as antitoxic serum was used on a somewhat larger scale. In his clinic, all the patients were examined bacteriologically, and serum was administered in every case of diphtheria without exception. Of 1336 cases treated before the serum-period, 554 = 39.4 per cent. died; whilst during the serum-period there were 55 deaths among 437 cases = 12 per cent. In cases of tracheotomy, the death-rates before and during the serum-period were 66 and 38.8 per cent. respectively." (*Lancet*, 7th May 1898, Report of German Surgical Congress at Berlin.)

"Dr. Kármán was entrusted by the Hungarian Government with the task of instituting measures for preventing the spread of diphtheria in a village and its neighbourhood. As general hygienic regulations accomplished nothing, he tried preventive inoculation. . . . Among 114 children thus treated, there was during the next two months no case of diphtheria, although the disease was prevalent in the village up to the date at which inoculation commenced, and continued to rage in the surrounding villages afterwards. During those two months, only one case of diphtheria appeared in the village, and that was in an uninoculated child; while, in the previous five months, 18.3 per cent. of the village children had been attacked, of whom eight died, six not having been treated with serum. Considering the wretched hygienic condition of the village, the harmlessness of preventive inoculations, and the continuance of the disease in the neighbouring villages, where diphtheria-vaccination was not carried out, the extraordinary value of the inoculations, in the prophylaxis of diphtheria, can hardly be denied." (*Brit. Med. Journ.*, 16th January 1897.)

"The most striking confirmation of the value of antitoxin has been afforded where the supply ran short during

an epidemic. In Baginsky's clinic, the interruption of the serum-treatment promptly raised the mortality from 15.6 to 48.4 per cent." (*Brit. Med. Journ.*, 20th October 1895.)

"In an analysis of the ratio of mortality in 266 German cities of about 15,000 inhabitants, it was found that the ratio of mortality per 100,000 of the living, before antitoxin was used, varied from 130 to 84 from 1886 to 1893, while the ratio from 1894 to 1897 varied from 101 to 35. It is a significant fact that during 1894, when, although antitoxin was used to a certain extent, it was not in general use, the ratio was 101; that when antitoxin was used more extensively, in 1895, the ratio was 53; that in 1896 it was 43; that in 1897, when antitoxin was very generally used, the rate fell to 35." (*Trans. Massachusetts Med. Soc.*, 1898.)

"Dr. Gabritchefski points out that in recent years the number of persons (in Russia) attacked by the disease has increased, the figures for the whole of Russia rising from about 100,000 or 120,000, ten years ago, to considerably over 200,000 in 1897. The introduction of the serum treatment has, however, had a marked effect on the mortality of the disease; and the actual number of deaths from diphtheria has either not increased at all, or has slightly diminished." (*Lancet*, 5th Aug. 1899.)

Of course there will still be bad diphtheria years and good diphtheria years: for example, the death-rate of the population of England, from diphtheria, was higher during the years 1893–1899 than during the years 1889–1892. Antitoxin can no more prevent a bad diphtheria year than an umbrella can prevent a wet day. But in limited outbreaks of diphtheria, such as occur in a village, an asylum, a school, or a large family of young children, it can be used, and is used, as a prophylactic, and with admirable results. The example of Dr. Kárman, just quoted, is one of the earliest instances of this

preventive use of antitoxin : other instances, of equal importance, are given in the *Boston Medical and Surgical Journal*, December 1897 and March 1898 ; and in the *Lancet*, 2nd April 1898, and 28th January 1899. A summary of later experiences of this preventive use of antitoxin in different countries is given by Dr. Wilcox of New York, and Dr. Stevens of Philadelphia, in Gould's *Year-Book* for 1902 :—

"At a meeting of the Société de Pédiatrie (Paris), held June 1901, a resolution was adopted affirming that preventive inoculations present no serious dangers, and confer immunity in the great majority of cases for some weeks, and recommending their employment in children's institutions and in families in which scientific surveillance cannot be exercised. Netter stated that he had collected 32,484 observations (cases) of prophylactic injections, and after eliminating cases in which the disease developed in less than twenty-four hours after injection, or more than thirty days after, there were 6 per cent. of failures. On the other hand, the author stated that he had recently made ninety preventive injections with but 2.17 per cent. of failures. Potter reports a series of twenty-four families in which preventive injections were used. Only one case of diphtheria occurred. In another series of cases, in which no prophylactic injections were given, the disease occurred secondarily in one-third of the houses, and one-sixth of the inmates contracted the disease, in spite of the fact that a large number of the primary cases were removed to the hospital. Blake reports a series of thirty-five prophylactic injections. The treatment was instituted after three cases of diphtheria had developed in a children's home. No secondary cases developed. Voisin and Guinon describe an epidemic of diphtheria in the Salpetrière Hospital among idiots and epileptics. Prophylactic injections were given to all those exposed to the contagion. After that, but four cases appeared, all mild in character. One severe case developed, however, two weeks later,

ending fatally in twenty-four hours, showing that the prophylactic action of the antitoxin, while efficacious, is not of very long duration."

It would be easy to prolong *ad infinitum* the proofs of the curative and preventive efficacy of the antitoxin: it would be impossible to find any evidence to be weighed for one moment against these proofs. There are three early records that ought to be quoted more fully: the 1894 report from the Hospital for Sick Children, Paris; the 1896 report of the American Pædiatric Society; and the 1898 report of the Clinical Society of London.

I

The report from the Hospital for Sick Children, Paris, is contained in a memoir, *Sérum-Thérapie de la Diphtérie*, the joint work of MM. Roux, Martin, and Chaillon (*Annales de l'Institut Pasteur*, September 1894). It gives the results of the serum-treatment during February to July 1894. The cases were not selected: the antitoxin was given in every case that was proved, by bacteriological examination, to be diphtheria—with the exception of 20 cases where the children were just dying when they were brought to the hospital. No change was made either in the general treatment or in the local applications to the throat; these were the same that had been used in former years: *le sérum est le seul élément nouveau introduit.*

In 1890–1893, before the serum-treatment, 3971 children were admitted to the diphtheria wards, and 2029 of them died. The percentage of these deaths was—

In 1890	55.88	Average = 51.71.
„ 1891	52.45	
„ 1892	47.64	
„ 1893	48.47	

send an inspector to every tenement-house case reported, and to administer the serum unless it was refused by the parents. These cases were therefore treated much earlier, and the results were correspondingly better than were obtained in New York, although the serum used was the same in both cities, viz., that of the New York Health Board."

The sum total of results was 5794 cases, with 713 deaths = 12.3 per cent., including every case returned; but 218 were moribund at the time of injection, or died within twenty-four hours of the first injection. "Should these be excluded, there would remain 5576 cases in which the serum may be said to have had a chance, with a mortality of 8.8 per cent.

Of	996	cases injected	on the first day of the disease,		49 died =	4.9 %
,,	1616	,,	on the second	,,	120 ,, =	7.4 ,,
,,	1508	,,	on the third	,,	134 ,, =	8.8 ,,
,,	758	,,	on the fourth	,,	147 ,, =	20.7 ,,
,,	690	,,	on or after the fifth	,,	244 ,, =	35.3 ,,

And in 232 cases, where the day of injection was unknown, there were 19 deaths = 8.2 per cent.

"No one feature of the cases of diphtheria treated by antitoxin has excited more surprise among the physicians who have reported them than the prompt arrest, by the timely administration of the serum, of membrane which was rapidly spreading downward below the larynx. Such expressions abound in the reports as 'wonderful,' 'marvellous,' 'in all my experience with diphtheria, have never seen anything like it before,' etc.

"Turning now to the operative cases, we find the same remarkable effects of the antitoxin noticeable. Operations were done in 565 cases, or in 16.7 per cent. of the entire number reported. Intubation was

performed 533 times, with 138 deaths, or a mortality of 25.9 per cent. In the above are included 9 cases in which a secondary tracheotomy was done, with 7 deaths. In 32, tracheotomy only was done, with 12 deaths, a mortality of 37.4 per cent. Of the 565 operative cases, 66 were either moribund at the time of operation or died within twenty-four hours after injection. Should these be deducted, there remain 499 cases operated upon, by intubation or tracheotomy, with 84 deaths, a mortality of 16.9 per cent.

"Let us compare the results of intubation, in cases in which the serum was used, with those obtained with this operation before the serum was introduced. Of 5546 intubation cases in the practice of 242 physicians, collected by M'Naughton and Maddren (1892), the mortality was 69.5 per cent. Since that time, statistics have improved materially by the general use (in and about New York, at least) of calomel fumigations. With this addition, the best results published (those of Brown) showed in 279 cases a mortality of 51.6 per cent.

"But even these figures do not adequately express the benefit of antitoxin in laryngeal cases. Witness the fact that over one-half the laryngeal cases did not require operation at all. Formerly, 10 per cent. of recoveries was the record for laryngeal cases not operated upon. Surely, if it does nothing else, the serum saves at least double the number of cases of laryngeal diphtheria that has been saved by any other method of treatment."

III

In 1898, the Clinical Society published the Report of their Special Committee, based on 633 cases (*Trans. Clin. Soc.*, xxxi., 1898, pp. 1–50). The whole report

should be read carefully; but there is room here for nothing more than the latter part of it. This is given at length.

A

Table showing the General Mortality of cases treated, on the same day of the disease, with and without Antitoxin.

Antitoxin Committee: 633 Cases treated with Antitoxin.				Metropolitan Asylums Board 1894: 3042 Cases treated without Antitoxin.				Difference of Percentage.
Day of the Disease on which Treatment was begun.	Cases.	Deaths.	Mortality per cent.	Day of Admission to Hospital.	Cases.	Deaths.	Mortality per cent.	
1st	20	2	10.0	1st	133	30	22.5	12.5
2nd	92	10	10.8	2nd	539	146	27.0	16.2
3rd	133	20	15.0	3rd	652	192	29.4	14.4
4th	130	26	20.0	4th	566	179	31.6	11.6
5th and after.	258	66	25.5	5th	1,152	355	30.8	5.3
Totals .	633	124	19.5	Totals .	3,042	902	29.6	10.1

B

Summary and Conclusions of the Committee's Report

"The material for the investigation of the clinical value of the antitoxin serum in the treatment of diphtheria was not obtained from selected, but from consecutive, cases, reported from the general hospitals and the fever hospitals of the Metropolitan Asylums Board; all were made use of which fulfilled the requirements of the Committee.

"The Committee rejected all cases in which satisfactory proof of the existence of true diphtheria was not shown, either by the presence of the *Bacillus diph-*

theriæ upon bacteriological examination, or by the occurrence of paralysis in the course of the illness. All were also rejected in which the amount of antitoxin administered was stated in cubic centimetres and not in normal units, the Committee having no means by which the strength of the antitoxin could in these cases be determined.

"Six hundred and thirty-three cases form the basis on which the report is drawn up; 549 were treated with antitoxin obtained from the laboratory of the Royal Colleges of Physicians and Surgeons; the remainder, 84 in number, were injected with antitoxin obtained from other sources. In nine instances, antitoxin from two different sources was injected into the same patient.

"Statistics of the disease before the use of antitoxin are introduced as control series; these were obtained from the fever hospitals of the Metropolitan Asylums Board, and from the general hospitals; and, like the antitoxin series, are compiled from consecutive and not from selected cases.

"The general mortality, under the antitoxin treatment, was 19.5 per cent.; a reduction of 10 on the percentage mortality of the cases treated in the hospitals of the Metropolitan Asylums Board in 1894. If 15 fatal cases, in which death took place within twenty-four hours of the first injection, be deducted, the mortality falls to 15.6 per cent.; which is very little more than half the mortality during 1894 under other forms of treatment.

"The lessened mortality is especially noticeable in the earlier years of life, the percentage mortality of children under five being 26.3, as opposed to 47.4. In the next period of five years, the percentage of

mortality is 16.0, as opposed to 26.0; whilst after ten years of age the difference in the mortality is slight.[1]

"Laryngeal diphtheria is admittedly the most dangerous form. The laryngeal cases have a percentage mortality of 23.6 in the antitoxin, as compared with 66.0 in the non-antitoxin series. In the cases in which laryngeal symptoms are so severe as to necessitate tracheotomy, the saving of life by the use of antitoxin is very marked, the mortality being reduced one-half, to 36.0 as opposed to 71.6 per cent.

"The strongest evidence of the value of the antitoxin treatment is that, in addition to reducing the general mortality by one-third, the duration of life in the fatal cases is decidedly prolonged. These two facts taken together conclusively prove the beneficial effects of the antitoxin treatment.

"The incidence of paralysis is greater in the antitoxin than in the control series. This increased number is partly explained by the lessened mortality, and partly by the longer duration of life in the fatal cases affording time for the development of paralytic symptoms. The percentage mortality of those who had some form or other of paralysis is lower in the antitoxin than in the control series; so that, notwithstanding the apparent greater risk of paralysis supervening, the probability of final recovery is greater.

"No definite conclusion can be drawn, for the reasons stated in the body of the report, as to the advantage of administering the whole of the antitoxin within forty-eight hours of the first injection, or continuing it for a longer period; but evidence is afforded of the importance of its administration as early as

[1] After childhood, the disease is much less fatal.

possible in the course of the disease; the percentage mortality in cases injected on the first and second days of the disease being 10.7, as compared with 25.5 for those first receiving the injection on the fifth or some subsequent day.

"No conclusion can be drawn, from the cases reported on, as to the amount of antitoxin which should be used to produce the best effects; but they show that the administration of very large doses is followed by no pronounced ill effects.

"The injection of antitoxin is responsible for the production of rashes, joint-pains, and possibly for the occurrence of late pyrexia. In 34.7 per cent. the injections were followed by rashes. Some amount of fever accompanied the rash in 60 per cent. In only 9.4 per cent. of those in whom rashes were observed did death ensue.

"Joint-pains were observed in 40, or 6.3 per cent. of the whole number, and all but five of them had a rash as well.

"In 26, or 65 per cent. of the joint-pains, some rise of temperature accompanied the pain. A rise of temperature during convalescence, accompanied by either rash or joint-pain, occurred in 27, or 4.2 per cent. of the whole number.

"No connection could be traced between the amount of antitoxin administered and the occurrence of rashes or late pyrexia, but the pain in and about the joints appears to have a relationship to the amount of antitoxin used.

"The results of the Committee's investigation tend to show that by the use of antitoxin—

1. The general mortality is reduced by one-third.
2. The mortality in tracheotomy falls by one-half.

3. Extension of membrane to the larynx very rarely occurs after the administration of antitoxin.

4. The duration of life in the fatal cases is decidedly prolonged.

5. The number of fatal cases is less when antitoxin is used early in the illness than in those which do not receive it until a later period.

6. The frequency of the occurrence of paralysis is not diminished, but the percentage of recoveries in cases with paralysis is slightly increased.[1]

7. Rashes are produced in about one-third of the cases, and are attributable to the antitoxin.

8. Pain, and occasionally swelling about the joints, are produced in a number of cases.

9. Even when used in large doses, no serious ill effects have followed the injection of antitoxin."

The foregoing reports belong to ancient history. Let us leave them, and study the record of the hospitals of the Metropolitan Asylums Board. They serve a city of 121 square miles, and 4½ millions of inhabitants.

[1] For an exhaustive and wise study of the diphtheritic paralyses, see Dr. Woollacott's essay in the *Lancet*, 26th August 1899: "The use of antitoxic serum in the treatment of diphtheria has, up to the present time, in the London fever hospitals, had two main results —the death-rate has fallen, while the paralysis-rate has risen. In the hospitals of the Metropolitan Asylums Board, the former has been reduced from 29 per cent. to 15.3 per cent., while the latter has risen from 13 per cent. to as high as 21 per cent. in 1896. This increase of paralysis is chiefly due to the fact that many more patients now recover from the primary disease, and live long enough for paralysis to show itself. *During the last two years, however, the occurrence of paralysis has begun to diminish in frequency. . . . The earlier antitoxin is given in diphtheria, the less likely is paralysis to follow.*" It is to be borne in mind that post-diphtheritic paralysis, in the great majority of cases, affects only a very small group of muscles; of Dr. Woollacott's tabulated cases, 377 were of this kind, and 97 were severe. And "the type of paralysis has, on the whole, become less severe, or at all events less dangerous to life."

The use of the antitoxin in the hospitals of the Metropolitan Asylums Board began in 1895. It had been used in 1894 on a few cases only, during the latter part of the year, and had been procured with much difficulty from various sources, chiefly from the Institute of Preventive Medicine. On 9th November 1894, the Board applied to the Laboratories' Committee of the Royal Colleges of Physicians and of Surgeons, asking them to undertake the supply. Arrangements were made for this purpose; and the sum of £1000 was given by the Goldsmiths' Company. Dr. Sims Woodhead, then Director of the Laboratories of the Conjoint Colleges, now Professor of Pathology at Cambridge, was put in charge of the bacteriological work and the preparation of the serum, with a host of expert colleagues: the administration of the treatment was the work of the medical officers of the hospitals of the Metropolitan Asylums Board. The experiences of 1895 are given in the following passages from the joint report to the Board from the medical superintendents:—

"The period covered by the report extends from 1st January 1895 to 31st December of the same year. During this time—with the exception of an interval of three months at the Eastern Hospital, when its use was suspended; of three months at the Fountain, and to a considerable extent throughout the year at the South-Eastern Hospital, when all cases were consecutively treated, irrespective of their severity—the serum was administered *only to cases which at the time of admission were severe, or which threatened to become so.* In a certain number, the patients being moribund at the time of their arrival, and beyond the reach of any treatment, no antitoxin was given. *No change has taken place during the year in the local treatment of the cases, nor*

has there been any new factor in the treatment other than the injection of antitoxin.

"It must be clearly understood that, with the exceptions previously stated, it has been the practice at each of the hospitals to administer serum to *those cases only in which the symptoms on admission were sufficiently pronounced to give rise to anxiety, the mild cases not receiving any.*

"*No less than* 46.4 *per cent. of the antitoxin cases were under five years of age, against* 32.5 *per cent. in the non-antitoxin group;* and only 16.1 per cent. in the former class were over ten years of age, against 33.8 per cent. in the latter. The high fatality of diphtheria in the earlier years of life is notorious.

"It is obvious, therefore, that to compare the mortality of those treated with antitoxin with that of those which during the same period were not so treated, would be to institute a comparison between the severe cases and those of which a large proportion were mild. This would clearly be misleading.

"The only method by which an accurate estimate can be obtained as to the merits of any particular form of treatment, is by comparing a series of cases in which the remedy has been employed with another series not so treated, but which are similar, so far as can be, in other respects. This, in the present instance, is impossible; but, having regard to the fact that 61.8 of the 1895 cases were treated with serum, an approximately accurate conclusion can be drawn by contrasting all cases of diphtheria completed during 1895, the antitoxin period, with all cases completed during 1894.

"The year 1894 has been selected for the purpose of comparison, not only because it is the year immediately preceding the antitoxin period, but because the average severity of the cases has been, in our opinion, about equal. Moreover, the death-rate in 1894 was slightly lower than it had been in any previous year.

". . . Of 3042 patients of all ages treated during 1894, 902 died—a mortality of 29.6 per cent.; whereas, of 3529

cases treated during 1895, 796 died—a mortality of 22.6 per cent.; the difference in percentage between the two rates being therefore 7.1. This, assuming that the former rate would otherwise have been maintained, represents a saving of 250 lives during the past year.

Influence of Age.

Table showing variations in reduction of mortality obtained with Antitoxin at different ages.

Ages.	Antitoxin Cases, 1895.			All Cases, 1895.			All Cases, 1894.			Diff. in Mortalities, 1894 and 1895.
	Cases.	Deaths.	Mortality per cent.	Cases.	Deaths.	Mortality per cent.	Cases.	Deaths.	Mortality per cent.	
Under 5 .	1013	379	37.4	1453	497	34.2	1171	556	47.4	13.2
,, 10 .	1829	575	31.4	2720	744	27.3	2246	836	37.2	9.9
,, 15 .	2056	606	29.4	3144	779	24.7	2609	877	33.6	8.9
All ages .	2182	615	28.1	3529	796	22.5	3042	902	29.6	7.1

For every age-group, with the single exception of that comprising the years 15 to 20 (the numbers of which are small), the percentage mortality was less in the 1895 than in the 1894 cases. The reduction in mortality was greatest in early life.

Influence of Time of coming under Treatment.

Table showing percentage mortality in relation to day of disease on which cases came under treatment.

Day of Disease.	1894.	1895.	Difference.
1st	22.5	11.7	10.8
2nd	27.0	12.5	14.5
3rd	29.4	22.0	7.4
4th	31.6	25.1	6.5
5th and over . .	30.8	27.1	3.7
Total . .	29.6	22.5	7.1

"It will be seen that the percentage mortality of cases admitted on the same day of disease is less in every

instance in the year 1895. The difference is most marked in the case of those patients who were admitted on the first and second day of illness, viz., 10.8 and 14.5 respectively.

"Both in 1894 and 1895, no less than over 37 per cent. of the patients were admitted on, or after, the fifth day of disease. And, moreover, while in 1894 as many as 59.2 per cent. of the fatal cases were not brought under treatment until the fourth day, or later, in 1895, the antitoxin year, the proportion was even higher, viz., 67.7 per cent.

Laryngeal Cases

"The tracheotomy results at each hospital are more favourable in the year 1895 than in 1894, the mortality ranging in the latter year at the different hospitals between 90 per cent. and 59.4 per cent., whereas in 1895 the range was from 56.2 to 40.5.

"The combined tracheotomy mortality for all the hospitals, which in 1894 was 70.4 per cent., has fallen to 49.4 per cent. in 1895. This is a lower death-rate than has ever been recorded in any single hospital of the Board for a year's consecutive tracheotomies. In other words, rather more than 50 per cent. of children on whom the operation has been performed have been saved since the employment of antitoxin. In one of the hospitals no less than a fraction under 60 per cent. survived, although the recoveries in that hospital in any previous year did not exceed 25 per cent., and in the preceding year—viz., 1894—were as low as 10 per cent.

"The improved results in the tracheotomy cases of 1895 have also been shared by analogous cases in which the operation was not performed. The percentage mortality of all laryngeal cases has fallen from 62 in 1894 to 42.3 in 1895.

"Moreover, the number of laryngeal cases which required tracheotomy has fallen in 1895 to 45.3 per cent., whereas in 1894 it was 56 per cent.

"The following tables briefly summarise the foregoing results. As no returns for 1894 were furnished by the Fountain Hospital by reason of the smallness of the numbers, the Fountain cases have also been omitted from the 1895 figures, in order that the two series may be rendered strictly comparable :—

1. *Comparative Mortality of Laryngeal Cases at all Hospitals, except the Fountain.*

Year.	Cases.	Deaths.	Percentage Mortality.
1894	466	289	62.0
1895	468	196	41.8

2. *Comparative Results in Tracheotomy Cases at all Hospitals, except the Fountain.*

Year.	Cases.	Deaths.	Percentage Mortality.
1894	261	184	70.4
1895	219	108	49.3

3. *Comparative Number of Laryngeal Cases which required Tracheotomy at all Hospitals, except the Fountain.*

Year.	Cases.	Tracheotomies.	Percentage of Tracheotomies.
1894	466	261	56.0
1895	468	219	46.8

"On these tables further comment seems unnecessary.

Summary

"The improved results in the diphtheria cases treated during the year 1895, which are indicated by the foregoing statistics and clinical observations, are—

1. A great reduction in the mortality of cases brought under treatment on the first and second day of illness.
2. The lowering of the combined general mortality to a point below that of any former year.
3. The still more remarkable reduction in the mortality of the laryngeal cases.
4. The uniform improvement in the results of tracheotomy at each separate hospital.
5. The beneficial effect produced on the clinical course of the disease.

Conclusions

"A consideration of the foregoing statistical tables and clinical observations, covering a period of twelve months, and embracing a large number of cases, in our opinion sufficiently demonstrates the value of antitoxin in the treatment of diphtheria.

"It must be clearly understood, however, that to obtain the largest measure of success with antitoxin it is essential that the patient be brought under its influence at a comparatively early date—if possible, not later than the second day of disease. From this time onwards, the chance of a successful issue will diminish in proportion to the length of time which has elapsed before the treatment is commenced. This, though doubtless true of other methods, is of still greater moment in the case of treatment by antitoxin.

"Certain secondary effects not unfrequently arise as a direct result of the injection of antitoxin in the form in which it has at present to be administered, and even assuming that the incidence of the normal complications of diphtheria is greater than can be accounted for by the

increased number of recoveries, we have no hesitation in expressing the opinion that these drawbacks are insignificant when taken in conjunction with the lessened fatality which has been associated with the use of this remedy.

"We are further of the opinion that in antitoxic serum we possess a remedy of distinctly greater value in the treatment of diphtheria than any other with which we are acquainted."

Now let us take the whole record of all the hospitals together. The disease was first admitted in 1888; this year is therefore to be reckoned as incomplete.

Year.	Percentage Mortality.	Year.	Percentage Mortality.
1888 . . .	59.35	1897 . . .	17.69
1889 . . .	40.74	1898 . . .	15.37
1890 . . .	33.55	1899 . . .	13.95
1891 . . .	30.63	1900 . . .	12.27
1892 . . .	29.35	1901 . . .	11.15
1893 . . .	30.42	1902 . . .	11.04
1894 . . .	29.29	1903 . . .	9.69
1895, first antitoxin year . .	22.85	1904 . . .	10.08
1896 . . .	21.20	1905 . . .	8.3

These results, of course, are but one instance of what has happened, since 1895, in every country all over the civilised world. *Securus judicat orbis terrarum.* We have Siegert's tables (1900), based on no less than 40,038 cases admitted in nine years to sixty-nine hospitals in Germany, Austria, Switzerland, and Paris. He divides these nine years into a "pre-serum period," an "introduction year," and a "serum period." In the pre-serum period the general mortality was 41.5, and

the mortality of cases requiring operation was 60; in the serum period, the general mortality was 16.5, and the mortality of cases requiring operation was 37.5.

Any bad results that have been recorded from the use of the antitoxin are so rare, in comparison with the hundreds of thousands of injections made, that they do not come to be considered here. And, even though a few have occurred, we may be sure that some of them were due, not to the antitoxin, but to the natural course of the disease.[1] The lesser drawbacks, the occurrence of joint pains and of rashes, are transient and in no way serious.

It has been supposed, and said, that the use of the antitoxin increases the complications of the disease. On this point, the best authority is Professor Woodhead's monumental Report (1901), dealing with the Metropolitan Asylums Board cases for 1895 and 1896. He sums up the matter thus :—

"The free use of antitoxin does not raise the percentage of cases of albuminuria. As regards vomiting, the statistics give little information, as vomiting is usually met with only in the very severe cases. This also holds good of anuria. The number of cases of adenitis appears to be distinctly reduced by the use of antitoxin, as the percentage of cases falls as the injections of antitoxin are pushed. The use of antitoxin has also had a perceptible effect in diminishing the cases of nephritis, and it certainly has not aggravated the kidney complications of diphtheria. There can be no doubt that in cases treated with antitoxin there is a greater percentage of cases in which joint-pains occur than in cases not so treated; these, however, are transitory, and are probably the

[1] This, of course, does not apply to two instances, in 1901, of accidental contamination of serum. See, for an account of these, *The British Medical Journal*, November 1901.

result of some slight change in the blood set up by the action of the serum itself, and not by the antitoxic substance in the serum. The number of primary abscesses has undoubtedly been reduced by the use of antitoxin. It may also be accepted that antitoxic serum has some effect in temporarily raising the temperature, but only during the periods of joint-pains and serum rashes; all these, however, are of comparatively slight importance as compared with the effect the antitoxin has in diminishing the percentage mortality and alleviating the more severe symptoms.

"It is of importance to observe that amongst the cases of paralysis following diphtheria the death-rate (32 per cent.) was actually higher amongst those not injected with antitoxin than amongst those where antitoxin was used (30.5 per cent.), although the former paralyses must be looked upon as being the result of a comparatively mild attack of the disease. From this it is evident that, when once paralysis supervenes in these cases, it is quite as fatal in its effects as in the cases (usually those of a more severe type) where antitoxin has been given. Antitoxin *cannot cure* the degeneration of the nerve, but it *can neutralise* the diphtheria toxin, and so put a stop to the advance of the degenerative changes due to its action. In 1896, when, of course, antitoxin was given much more freely, the percentage of deaths in the non-injected cases where paralysis had come on fell to 18.4.

"Antitoxin rashes occur at a comparatively late stage of the disease. They cannot be looked upon as in any way dangerous, although the secondary rise of temperature, and the irritation of the skin which usually accompany their presence are very undesirable complications, and may retard somewhat the convalescence of nervous and irritable patients.

"Antitoxin appears to diminish the liability of the lungs to inflammatory change in severe attacks of diphtheria."

Now let us take another point of view. If anybody really doubts whether the antitoxin did really save these lives in the hospitals of the Metropolitan Asylums Board, what answer has he got to the following table? It is published in the Board's Report for 1904, and was drawn up by Dr. MacCombie, Medical Superintendent of the Brook Hospital. It shows the supreme importance of giving the antitoxin *at the very beginning of the disease.* The figures in brackets are the total numbers of the cases in the eight years :—

Percentage Mortality according to Time of coming under Treatment.

Day of Disease.	1897.	1898.	1899.	1900.	1901.	1902.	1903.	1904.
(204) 1st .	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0
(1278) 2nd .	5.4	5.0	3.8	3.6	4.1	4.6	4.2	5.43
(1374) 3rd .	11.5	14.3	12.2	6.7	11.9	10.5	17.6	10.63
(1086) 4th .	19.0	18.1	20.0	14.9	12.4	19.8	16.7	19.51
(1382) 5th and after .	21.0	22.5	20.4	21.2	16.6	19.4	17.3	13.11

Here we see that in 1482 patients, who got the antitoxin within forty-eight hours of the onset of the disease, the mortality was $2\frac{1}{4}$ per cent. In 1278 patients, who did not get the antitoxin till the third day, the mortality was $11\frac{3}{4}$ per cent. That is the result of one day's delay over sending the child into hospital.

Again, it is not only lives that are saved, but suffering that is avoided. Just lately, at a meeting of the Chelsea Clinical Society (May 1906), reference was made to this point by Dr. Foord Caiger, Medical Superintendent of the South-Western Hospital. "The number of tracheotomies is less than half what it used

to be;" and again, "Instead of the spectacle of a number of patients in great distress, with swollen necks and stuffed-up noses, fretful and crying, such cases are now quite the exception, and, in the few one does come across, the condition lasts for a comparatively short time." And again, "It was quite unusual (before 1895) for a nurse to care to stay very long in charge of one of the diphtheria wards, because she found the work so depressing. But nowadays the diphtheria wards are perhaps the most popular in the hospital, a fact which is mainly owing to the change in the general aspect of the patients and the greatly reduced mortality." (*Clinical Journal*, May 23, 1906.)

the ground and gets an abrasion of the skin, all tillers of the soil who get accidental wounds in the course of duty, and every horse which 'breaks its knees' by falling in the London streets, runs potentially a risk of inoculation with tetanus." Nicolaier therefore studied the various microbes of the soil, and made inoculations of garden-mould under the skin of rabbits. He was able, by these inoculations, to produce tetanus in them ; and the discharge from the points of inoculation, put under the skin of other rabbits, produced the disease again. He also identified the bacillus, and cultivated it ; but in these cultures it was mixed with other organisms, and he failed to isolate it from them. Carle and Rattone, and Rosenbach, were able to produce tetanus in animals by inoculating them with discharge from the wounds of patients attacked by the disease. Finally, Kitasato, in 1889, found a way of obtaining pure cultures of the bacillus. Beginning with impure cultures such as Nicolaier had made, he kept these at a temperature of 36° C. till the bacillus had spored ; then, by repeated exposures of the cultures to a temperature of 80° C. for three-quarters of an hour at a time, he killed-off all organisms except the spores of the tetanus-bacillus ; then he kept these in an atmosphere of hydrogen, at a temperature of 20° C., and thus got pure cultures.

Brieger, Fränkel, Cohen, Sidney Martin, Kanthack, and others, have studied the chemical products of the disease, have obtained them from cultures and from infected tissues, and have been able with these toxins to produce the disease in animals. As with the other infective diseases, so with tetanus, there have been two main lines of researches ; the one, toward a fuller knowledge of the chemical changes in the blood and in

the central nervous system; the other, toward a fuller knowledge of the nature and ways of the bacillus, and its method of invasion. Before any study of immunity or immunisation, or of neutralisation of the toxins in man by an antitoxin, came the study of the toxins and of the bacillus. It was proved, by an immense quantity of hard work, that the bacillus does not tend to invade the blood, or to pass beyond the lymphatic glands in the immediate neighbourhood of the site of inoculation; that it stays in and about the wound, and there multiplies, and from this site pours into the blood the chemical products which cause the disease; and that these chemical substances have a selective action on certain nerve-cells in the brain and the spinal cord. This is the bare outline of the facts; and no account can be given here of the intricate problems of bacteriology and animal chemistry that have been answered, or are still waiting an answer. At least, it is evident that the whole pathology of tetanus was found, proved, and interpreted by the help of experiments on animals; and that these alone did away with the old false doctrine that the disease was due to rapid extension of inflammation up a nerve to the brain.

In 1894 came the use of an antitoxin in cases of the disease, and, in 1895, 42 cases were reported, with 27 recoveries. It cannot be said that any one of the diverse preparations of tetanus-antitoxin, up to this present time, has triumphed over the disease. Tetanus is of all diseases the hardest to reckon with: the first sign of it is the last stage of it; there is no warning, nothing, it may be, but a healed scratch, till the central nervous system is affected with sudden and rapidly advancing degeneration of certain cells. These and other difficulties have stood in the way of an antitoxin

treatment; and there is no less difficulty in estimating the efficacy of that treatment. The recovery, under antitoxin, of a "chronic" case cannot always or altogether be attributed to the treatment; and in a very acute case, antitoxin, like everything else, has but small chance of success. Various reports on the antitoxin treatment, published during 1897–1899, give the following figures:—

26	cases, with	12	recoveries.
98	,,	57	,,
36	,,	25	,,
22	,,	11	,,
51	,,	36	,,
10	,,	7	,,

Probably the paper by Dr. Lambert of New York, in the *Medical News*, July 1900, gives fairly the general opinion of the treatment, so far as the subcutaneous administration of antitoxin is concerned:—

"The following cases of tetanus, treated with antitoxin, comprise published and unpublished cases. We have a total of 279 cases, with a mortality of 44.08 per cent.: but of these we must rule out 17 cases—4 deaths from intercurrent diseases, 8 deaths in cases in which the antitoxin was given but a few hours before death, and 5 recoveries in which antitoxin was not given until after the twelfth day (as they probably would have recovered without it). We have left 262 cases, with 151 recoveries, and 111 deaths, a mortality of 42.36 per cent. Dividing the cases into acute and chronic, we have 124 acute cases, with 35 recoveries and 89 deaths, a mortality of 71.77 per cent., and 138 chronic cases, with 116 recoveries and 22 deaths, a mortality of 15.94 per cent. In interpreting critically these statistics, we see that in acute cases the mortality is but slightly reduced, being but 72 per cent. instead of 88 per cent. But, in the less acute cases, there is a decided improvement, from 40 per cent. to 16 per cent. Taking the statistics as a whole, there

is a distinct improvement in the mortality of tetanus since the introduction of antitoxin."

It would be foreign to the present purpose to pursue this matter further: for the other treatments, used by Baccelli and by Krokiewicz, and the sub-dural use of antitoxin, are also founded on experiments on animals; and the same will be true of any better method that shall be developed out of them.

The *preventive* use of the tetanus-antitoxin, for the immunisation of human beings or of animals, has given excellent results. Horses are very apt to be infected by tetanus; and the antitoxin has been used in veterinary practice, both for prevention and for cure. The curative results are not, at present, very good. But, as regards protection against the disease, there is evidence that horses can be immunised against tetanus by the antitoxin with almost mechanical accuracy. In some parts of the world, the loss of horses by tetanus is so common that their immunity is a very important matter; and that the antitoxin does confer immunity on them is shown by statistics from France and from the United States:—

1. *France.*—"The results of Nocard's method of preventive inoculations in veterinary practice are most striking. Among 63 veterinarians, there have been inoculated 2737 animals with preventive doses of antitoxin, and not a single case of tetanus developed; while during the same period, in the same neighbourhoods, 259 cases of tetanus developed in non-inoculated animals." (*Med. News*, 7th July 1900.)

2. *United States.*—"Joseph MacFarland and E. M. Ranck, in addition to a synopsis of the method of manufacture of tetanus-antitoxin, give some facts of interest and importance in regard to its use for prophylaxis and treatment. The studies were made upon several

hundred horses used for the production of various immunised serums in one of the large laboratories of the United States. The horses, because of the constant manipulations, frequently became infected with tetanus, and in 1897 and 1898, when scrupulous cleanliness and disinfection were the only precautions employed to prevent the disease, the death-rate varied from 8 to 10 per cent. During 1899 nearly two hundred horses were subjected to systematic immunisation with tetanus-antitoxin; and, in spite of otherwise similar conditions, the death-rate descended to 1 per cent." (*Medical Annual*, 1901.)

The preventive use of the antitoxin has, of course, a very limited range outside veterinary surgery. Tetanus, thanks to the use of antiseptic or aseptic methods, not only in hospital surgery but also in amateur and domestic surgery, has become a very rare disease, except in tropical countries. It is no longer a "hospital disease"; and, even in war, it no longer has anything like the frequency that it had, for instance, in the War of the Rebellion. A student may now go all his time at a large hospital without seeing more than a very few cases. But, now and again, attention is called to some wholly unsuspected risk of the disease. For example, certain cases of tetanus occurred in Dundee among workers at the jute-mills there :—

"The last victim was a female worker in the jute-mill, who, six days after a crushed and lacerated wound of the foot, developed tetanus and died within twenty-four hours. Some of the dust, taken from under the machine in which the foot was crushed, was found to contain an unusually large number of tetanus-bacilli. The source of the jute used is India." (*Medical News*, August 1900.)

Again, at the Gebaer Anstalt at Prague, in 1899, an outbreak of tetanus occurred, with several deaths; but

it was stopped when a preventive dose of the antitoxin was given to the new patients on admission.

Again, an amazing number of deaths from tetanus, in the United States, are due to wounds of the hands with toy-pistols. It is said that after the Fourth of July festivities in 1899, no less than 83 cases of tetanus were reported, 26 of them in and around New York. Almost all of them were due to gunshot wounds of the hand with toy-pistols: the unclean wad of the cartridge, made of refuse paper picked up in the streets, penetrates deep into the tissues of the hand, taking the germs of the disease with it, out of the reach of surgical disinfection. These cases of tetanus in the United States from toy-pistol wounds are so frequent, that immunisation has been recommended for them. The *Medical News*, 1st June 1901, has the following note:—"H. G. Wells states that tetanus is endemic in Chicago, the specific organism being present in the dirt of the streets. Every Fourth of July an epidemic occurs, because these bacilli are carried deeply into wounds before wads from blank cartridges. . . . The writer thinks that such cases should receive a prophylactic dose, say, 5 c.c. of tetanus-antitoxin, as soon as possible after the wound is first seen. It seems certain that if antitoxin prophylaxis were adopted, there would be no further Fourth of July epidemics, and this end would justify the means."

Again, a man might receive a lacerated wound under conditions especially favourable to infection: he might tear his hand in a stable where horses had died of tetanus, or he might cut his finger while he was working at the disease in a pathological laboratory, or he might receive a poisoned arrow-wound out in Africa. In any such emergency, he could safeguard his life with a protective dose of antitoxin.

Pasteur was led to use, instead of the saliva, an emulsion of the brain or spinal cord; because, as Dr. Duboué had suggested, the central nervous system is the chief seat, the *locus electionis*, of the virus of rabies. But these inoculations also were not always successful, nor did they give a definite incubation-period.

Therefore he followed with rabies the method that he had followed with anthrax. As he had cultivated the virus of anthrax, by putting it where its development could be watched and controlled, so he must put the virus of rabies in the place of its choice. It has a selective action on the cells of the central nervous system, a sort of affinity with them; they are, as it were, the natural home of rabies, the proper nutrient medium for the virus: therefore the virus must be inoculated not under the skin, but under the skull.

These sub-dural inoculations were the turning-point of Pasteur's discovery. The first inoculation was made by M. Roux:—

"Next day, when I informed Pasteur that the intracranial inoculation offered no difficulty, he was moved with pity for the dog. 'Poor beast, his brain is doubtless injured: he must be paralysed.' Without reply I went down to the basement to fetch it, and let it come into the laboratory. Pasteur did not like dogs, but when he saw this one, full of life, inquisitively rummaging about in all directions, he exhibited the greatest delight, and lavished most charming words upon it."

Henceforth all uncertainty was at an end, and the way was clear ahead: Pasteur had now to deal with a virus that had a definite period of incubation, and a suitable medium for development. The central nervous system was to the virus of rabies what the test-tube was to the virus of fowl-cholera or anthrax. As he

had controlled these diseases, had turned them this way and that, attenuated and intensified them, so he could control rabies. By transmitting it through a series of rabbits, by sub-dural inoculation of each rabbit with a minute quantity of nerve-tissue from the rabbit that had died before it, he was able to intensify the virus, to shorten its period of incubation, to fix it at six days. Thus he obtained a virus of exact strength, a definite standard of virulence, *virus fixe*: the next rabbit inoculated would have the disease in six days, neither more nor less.

As he was able to intensify the virus by transmission, so he was able to attenuate it by gradual drying of the tissues that contained it. The spinal cord, taken from a rabbit that has died of rabies, slowly loses virulence by simple drying. A cord dried for four days is less virulent than one that has been dried for three, and more virulent than one dried for five. A cord dried for a fortnight has lost all virulence: even a large dose of it will not produce the disease. By this method of drying, Pasteur was enabled to obtain the virus in all degrees of activity: he could always keep going one or more series of cords, of known and exactly graduated strengths, according to the length of time they had been dried—ranging from absolute non-virulence through every shade of virulence.

And, as with fowl-cholera and anthrax, so with rabies; a virus which has been attenuated till it has been rendered innocuous, can yet confer immunity against its more virulent forms: just as vaccination can protect against small-pox. A man, bitten by a rabid animal, has at least some weeks of respite before the disease can break out; and, during that time of respite, he can be immunised against the disease, while it is

still dormant: he begins with a dose of virus attenuated past all power of doing harm, and advances day by day to more active doses, guarded each day by the dose of the day before, till he has manufactured within himself enough antitoxin to make him proof against any outbreak of the disease.

The cords used for treatment are removed from the bodies of the rabbits, by an aseptic method, and are cut into lengths and hung in glass jars, with some chloride of calcium in them, for drying. The jars are dated, and then kept in glass cases in a dark room at a constant temperature. To make sure that the cords are aseptic, a small portion of each cord is sown on nutrient jelly in a test-tube, and watched, to see that no bacteria occur in the tube. For each injection, a certain small quantity of cord is rubbed-up in sterilised fluid; and these subcutaneous injections give no pain or malaise worth considering.

Of course, the treatment is adjusted to the gravity of the case. A bite through naked skin is more grave than a bite through clothing; and bites on the head or face, and wolf-bites, are worst of all. The number and character of the scars are also taken into account. An excellent description of the treatment, by a patient, was published in the *Birmingham Medical Review* of January 1898. It gives the following tables of treatment:—

1. *Ordinary Treatment.*

Day of Treatment.	Days of Drying of Cord.	Day of Treatment.	Days of Drying of Cord.
1	14 and 13	9 (½ dose)	3
2	12 and 11	10 (full dose)	5
3	10 and 9	11	5
4	8 and 7	12	4
5	6	13	4
6	6	14 (½ dose)	3
7	5	15 (full dose)	3
8	4		

2. *Cases of Moderate Gravity.*

Same treatment, up to 13th day.

Day of Treatment.	Days of Drying of Cord.	Day of Treatment.	Days of Drying of Cord.
14	3	17 (½ dose)	3
15	5	18 (full dose)	3
16	4		

3. *Grave Cases.*

Same treatment, up to 10th day.

Day of Treatment.	Days of Drying of Cord.	Day of Treatment.	Days of Drying of Cord.
11	4	17 (½ dose)	3
12	3	18 (full dose)	3
13	5	19	5
14	5	20	3
15	4	21	4
16	4	22	3

4. *Very Grave Cases.*

Same treatment as 3, and in addition.

Day of Treatment.	Days of Drying of Cord.	Day of Treatment.	Days of Drying of Cord.
23	5	25 (½ dose)	3
24	4	26 (full dose)	3

Furious criticism, unbelief, and flagrant misstatement of facts began at once, and lasted more than two years. Of Pasteur's opponents, the chief was M. Peter, who besought the Académie des Sciences, about once a week, that they should close Pasteur's laboratory, because he was not preventing hydrophobia but producing it. The value of M. Peter's judgment may be estimated by what he had said, a few years earlier, about bacteriology in general—"I do not much believe in that invasion of parasites which threatens us like an eleventh plague of Egypt. After so many laborious researches, nothing will be changed in medicine, there will only be a few more microbes. M. Pasteur's excuse is that he is a

chemist, who has tried, out of a wish to be useful, to reform medicine, to which he is a complete stranger."

But it does not matter what was said twenty years ago. In England, the Report of the 1886 Committee, and the Mansion House meeting in July 1889, mark the decline and fall of all intelligent opposition to the work. Among so many thousand cases, during so many years, it would be a miracle indeed if not a single case had failed or gone amiss; but we are concerned here with the thousands. Take, to begin with, four reports from Athens, Palermo, Rio, and Paris. It is to be noted that the patients, alike at Paris and at other Institutes, are divided into three classes:—

"A. Bitten by animals proved to have been rabid by the development of rabies in other animals inoculated from them.

"B. Bitten by animals proved to have been rabid by dissection of their bodies by veterinary surgeons.

"C. Bitten by animals suspected to have been rabid."

It is to be noted also, as a fact proved beyond doubt, that the full benefit of the treatment is not obtained at once; the highest degree of immunity is reached about a fortnight after the discontinuance of the treatment. Those few cases, therefore, where hydrophobia has occurred, not only in spite of treatment, but within a fortnight of the last day of treatment, are counted as cases where the treatment came too late.

Finally, what was the risk from the bite of a rabid animal, in the days before 1885? It is a matter of guess-work. One writer, and one only, guessed it at 5 per cent.; another guessed it at 55, and a third came to the safe conclusion that it was "somewhere between these limits." Leblanc, who is probably the best

guide, put it at 16; and Pasteur himself put it between 15 and 20. But suppose it were only 10; that, before Pasteur, out of every 100 men bitten by rabid animals, 90 would escape and only 10 would die of hydrophobia; then take this fact, that in one year, at one Institute alone, there were 142 patients in class A, bitten by animals that were proved, by the unanswerable test of inoculation, to have been rabid; and 1 death. And every year the same thing; and in all the twelve years together, 2872 such cases (A) and 20 deaths—a mortality not of 10 per cent., but of less than 1 per cent.

1. *Athens*

The *Annales de l'Institut Pasteur*, June 1898, contain Dr. Pampoukis' report of three years' work at the Hellenic Institute, from August 1894 to December 1897. During this period 797 cases were treated—590 male and 207 female. The animals that bit them were—dogs, 732; cats, 34; wolf, 1; other animals, 13; and the 17 other patients had been exposed to infection from the saliva of hydrophobic patients. Of the 797 cases, 245 were of class A, 112 B, and 440 C.

"Among the 797 persons treated, there are 2 deaths, one in class B and the other in class C. Thus the mortality has been 0.25 per cent. Besides these 2 who died of rabies there are 5 more, in whom the first signs of rabies showed themselves in less than fifteen days after the last inoculation.

"Finally, beside these 797 cases, there is 1 other case, bitten by a wolf, in which the treatment failed. If we reckon this last case in the statistics of mortality, we have 3 deaths in 798 cases = 0.37 per cent.

"Beside these 798 cases treated at the Institute,

there have been others that have not undergone the antirabic treatment, having trusted the assurances of those who are called in Greece *empirics*. Among these non-treated cases there are 40 who have died of rabies."

2. *Palermo*

The *Annales* for April 1896 give the report by Dr. de Blasi and Dr. Russo-Travali of the work of the Municipal Institute at Palermo during 8½ years, from March 1887 to December 1895. The number of cases was 2221; in 1240 (class A), the animals were proved to have been rabid by the result of inoculations; in 981, there was reason to suspect rabies.

"Setting aside 5 patients who died during the course of the treatment, and 5 others who died less than fifteen days after the end of the treatment, we have had to deplore only 9 failures = 0.4 per cent. Even if we count against ourselves the 10 other cases, the mortality is still only 0.85."

3. *Rio de Janeiro*

The *Annales* for August 1898 give Dr. Ferreira's report of ten years' work (February 1888 to April 1898) at the Pasteur Institute at Rio. The number of cases treated was 2647, of whom 1987 were male and 660 female. Beside these 2647 there were 1234 who were not treated, because it was ascertained that they were in no danger of rabies; 3 who were brought to the Institute, already suffering from the disease; and 59 who refused treatment.

Of the 2647 persons treated, 10 had pricked their hands at work in the laboratory, 3 had exposed chance scratches on their hands to the saliva of rabid animals,

and 1 had been bitten by a rabid patient. Of the rest, 1886 had been bitten on the bare skin, and 747 through clothing.

In 236 cases the rabies of the animal had been proved by inoculation. In 1173 it had been recognised by the signs of the disease. In 1238 there was good reason to suspect that the animal had been rabid.

Of the 2647 patients, in 30 cases the treatment was stopped, because the animals were at last traced, after treatment was begun, and were found not to be rabid. In 65 cases the patients, after treatment was begun, refused to go on with it, and 3 of them died of rabies. In 6 cases rabies developed during treatment; 5 of them had been very badly bitten about the head, and 1 did not come for treatment till the twenty-first day after the bite, and was attacked by rabies two days later. And 5 cases died of other maladies that had nothing to do with rabies. Setting aside these 106 cases, there remain 2541 cases, with 20 deaths = 0.78 per cent. But, of these 20 deaths, 9 occurred within fifteen days of the end of treatment, before protection was fully established. If these 9 deaths be excluded, the figures stand at 2532 cases, with 11 deaths = 0.43 per cent.

4. *Paris*

Dr. Pottevin's report on the work of the Pasteur Institute (Paris) during 1897 (*Annales*, April 1898) must be given word for word, without abbreviation.

I

During 1897, 1521 patients received the anti-treatment at the Pasteur Institute: 8 died of rabies.

The notes of their cases will be found at the end of this paper.

If we exclude 2 of these 8 cases—the cases of Heniquet and Morin, where death occurred before it was possible for the vaccinations to produce their effect—the results of the vaccinations in 1897 are

Patients treated . . .	1519
Deaths	6
Mortality per cent. . . .	0.39

In the following table these figures are compared with those of preceding years :—

Year.	Patients treated.	Deaths.	Mortality per cent.
1886	2671	25	0.94
1887	1770	14	0.79
1888	1622	9	0.55
1889	1830	7	0.38
1890	1540	5	0.32
1891	1559	4	0.25
1892	1790	4	0.22
1893	1648	6	0.36
1894	1387	7	0.50
1895	1520	5	0.33
1896	1308	4	0.30
1897	1521	6	0.39

II

Patients treated at the Pasteur Institute are divided into three classes, as follows :—

A. The rabies of the animal was proved by experiment, by the development of rabies in animals inoculated with its bulb (the upper end of the spinal cord).[1]

[1] It is satisfactory to know that rabbits affected with rabies do not suffer in the same way as dogs and some other animals, but become subject to a painless kind of paralysis.

B. The rabies of the animal was proved by veterinary examination (dissection of its body).

C. The animal was suspected of rabies.

We give here the patients treated in 1897, under these three classes :—

	Bites of the Head.			Bites on the Hands.			Bites of the Limbs.			Total.		
	Patients.	Deaths.	Mortality per cent.	Patients.	Deaths.	Mortality per cent.	Patients.	Deaths.	Mortality per cent.	Patients.	Deaths.	Mortality per cent.
A	15	0	0	81	0	0	46	1	2.1	142	1	0.7
B	106	0	0	539	4	0.74	273	1	0.4	918	5	0.65
C	30	0	0	244	0	0	187	0	0	461	0	0
	151	0	0	864	4	0.46	506	2	0.4	1521	6	0.39

The following tables, giving the results obtained since the vaccinations were first used, show that the gravity of the bites varies with their position on the body, and that the mortality is always below 1 per cent. among patients bitten by dogs undoubtedly rabid :—

	Patients.	Deaths.	Mortality.		Patients.	Deaths.	Mortality.
Bites of the Head .	1,759	21	1.1	A	2,872	20	0.69
Bites of the Hands .	11,118	53	0.47	B	12,547	61	0.48
Bites of the Limbs .	7,289	22	0.30	C	4,747	15	0.31
	20,166	96	0.46		20,166	96	0.46

III

In regard to their nationality, the 1521 patients treated at the Pasteur Institute in 1897 were as follows :—

Germany	8	United States	1
England	83	Greece	1
Belgium	14	India	33
Egypt	2	Switzerland	33

That is, 175 foreigners and 1346 French.

IV

Notes of the eight cases where the treatment failed :—

1. Camille Bourg, 26. Bitten 11th April; treated at the Pasteur Institute, 13th to 30th April; died of rabies at the Lariboisière Hospital, 26th May. Six penetrating bites on the ball of the left thumb. The dog was examined by M. Grenot, a veterinary surgeon at Paris, and the dissection gave evidence of rabies. Another person bitten and treated at the same time as Bourg is now in good health.

2. Louis Fiquet, 23. Bitten 22nd April; treated at the Pasteur Institute, 23rd April to 10th May; died of rabies at the Necker Hospital, 4th June. Five bites, two of them deep, round the right thumb. They had been cauterised five hours after infliction. The dog was examined by M. Caussé, a veterinary surgeon at Boulogne, and the dissection gave evidence of rabies. Another person bitten at the same time as Fiquet is now in good health.

3. Annette Beaufort, 19. Licked on the hands, which were chapped, on 15th April. The dog was killed next day, examined, and declared to have been rabid by M. Lachmann, a veterinary surgeon at Saint-Étienne. Treated at the Pasteur Institute, 20th April to 7th May.

Died of rabies 14th October. Two other persons bitten by the same dog and treated at the Pasteur Institute are now in good health.

4. Julien Heniquet, 53. Bitten 11th March, by a dog that M. Jenvresse, veterinary surgeon at Beaumont-sur-Oise, declared after dissection to have been rabid. One bite had torn the lower lip, the wound had been sutured; three other wounds on the nose. The wounds had not been cauterised. Treated at the Pasteur Institute, 18th May to 5th June. First symptoms of rabies showed themselves 4th June, before the treatment was finished; died 7th June. As the disease had its onset during the course of the inoculations, this case should be excluded from the number of those who died of rabies after treatment.

5. Germain Segond, 7. Penetrating bite on the bare right fore-arm, 23rd May. Cauterised an hour later with a red-hot iron. Treated 26th May to 9th June; died of rabies 22nd July. The dog's bulb had been sent to the Pasteur Institute. A guinea-pig inoculated in the eye 26th May was seized with rabies 10th September.

6. Suzanne Richard, 8. Bitten 12th June on the left leg by a dog, found on dissection to have been rabid by M. Touret, veterinary surgeon at Sannois. The bite, penetrating 3 cm. long, had been sutured; it had been made through a cotton stocking, and had been cauterised in half-an-hour. Treated 13th to 30th June; died of rabies 2nd August. (Notes from M. le Dr. Margny, at Sannois.)

7. Joseph Vaudale, 33. Bitten on the left hand, 8th August. Six penetrating bites on the back of the hand; had not been cauterised. The dog was declared rabid by M. Verraert, veterinary surgeon at Ostend. Treated at the Pasteur Institute, 11th to 28th August; died of rabies 27th September.

8. Paul Morin, 38. Bitten 24th August on the left cheek, a single bite, 2 cm. long; no cauterisation. The dog was sent to the Alfort School, 25th August, and found to be rabid. Treated at the Pasteur Institute,

26th August to 15th September. Died of rabies some days after the end of treatment (three weeks after the bite, says a note sent to us). The interval between the end of the treatment and the onset of the disease being less than fourteen days, Morin must not be counted in the number of patients inoculated under conditions which permit successful inoculation.

We hardly need follow the work of the remaining years. The figures are as follows :—

Year.	Patients treated.	Deaths.	Mortality per cent.
1898	1465	3	0.2
1899	1614	4	0.25
1900	1420	4	0.28
1901	1318	5	0.38
1902	1105	2	0.18
1903	628	2	0.32
1904	755	3	0.39

The falling off in the number of patients at the Paris Institute is related to the establishment of similar Institutes at Lyon, Marseilles, Bordeaux, Lille, and Montpellier. But is it not possible that a patient, after treatment at the Paris Institute, should die at home of rabies, and his death not be notified to the Institute? The answer is, that the Institute is very careful, so far as possible, to keep in touch with its old patients. For instance, in 1903, it recorded the case of a carpenter in a Welsh village, who had died of rabies nearly two years after treatment. And, of course, an Institute patient, wherever he was, would be of interest to his neighbours: and a death from rabies would excite attention, and would hardly fail to be reported.

It is not impossible that some sort of intensive modification of Pasteur's treatment may be found, not for the prevention, but for the cure of hydrophobia; and two successful cases of this kind have been reported in the *Annales* of the Paris Institute. Apart from this faint hope, the *cure* of hydrophobia is where it was in the days of the "Tonquin medicine" and the "Tanjore pills."

VII

CHOLERA

THE study of cholera was the hardest of all the hard labours of bacteriology; it took years of work in all parts of the world, and the difficulty and disappointments over it are past all telling. Koch's discovery of the comma-bacillus (1883) raised a thousand questions that were solved only by infinite patience, international unity for science, and incessant research; and the Hamburg epidemic (1892) marks the time when the comma-bacillus was at last recognised as the cause of cholera. A mere list of the men who did the work would fill page after page; it was bacteriology *in excelsis*, often dangerous,[1] and always laborious.

There is the same heroic note in the story of the preventive treatment of cholera by Haffkine's method; one of the men in whom Pasteur seems to live again. He began in 1889, under Pasteur's guidance, to study

[1] "In order to prove that this *vibrio* is the cause of Asiatic cholera, several tests upon themselves have been voluntarily made by investigators in laboratories. These were carried out in Munich and in Paris. The results to the experimenters were sufficiently severe to indicate positively the pathogenic character of the spirillum, and its capacity to produce cholera-like infections. Such experimentation is, of course, to be deprecated; indeed, the occurrence of accidental laboratory infections, one of which ended fatally, furnished the necessary final proof of the specificity of the cholera *vibrio*, and rendered unnecessary any exposure to the risks belonging to voluntary inoculation." (Dr. Flexner, Stedman's *Twentieth Century Practice*, vol. xix., 1900.)

the immunisation of animals against the cholera-bacillus. Other men, of course, were working on the same lines —Pfeiffer, Brieger, Metchnikoff, Fischer, Gamaleïa, Klein, Wassermann, and many more—and by 1892 the immunisation of animals was proved up to the hilt. Then came the advance from animals to men, from laboratories to Indian cities, villages, and cantonments; and here the honour is Haffkine's, and his alone. Ferran's inoculations (Spain, 1885) had failed. Haffkine, having tested his method on himself and his friends, went to India, with a commendatory letter from the British Government:—

"Researches on cholera, with special reference to inoculation, were undertaken and carried on in my laboratory, in the Pasteur Institute in Paris, between 1889 and 1893. The experiments resulted in the elaboration of the present method, which when tried on animals was found to render them resistant against every form of cholera-poisoning otherwise fatal to them.

"The physiological and pathological effect on man was then studied on some sixty persons, mostly medical and scientific men interested in the solution of the problem. The effect was found to be harmless to health. The next step was to transfer the operations to the East." (Haffkine's *Report to the Government of India*, 1895.)

He reached Calcutta in March 1893, and at the request of Mr. Hankin[1] was invited to Agra; here, in April, he vaccinated over 900 persons, including many English officers. From Agra to Aligarh; and from Aligarh he was asked to more places than he could

[1] Mr. Hankin, whose name is had in remembrance by Cambridge men, is Chemical Examiner and Bacteriologist to the North-West Provinces and Oudh, and to the Central Provinces.

visit. In 1895 his health failed, and no wonder; and he came back to Europe for a short time:—

"My actual work in India lasted twenty-nine months, between the beginning of April 1893 and the end of July 1895. During this period the anti-cholera vaccination has been applied to 294 British officers, 3206 British soldiers, 6629 native soldiers, 869 civil Europeans, 125 Eurasians, and 31,056 natives of India. The inoculated people belonged to 98 localities in the North-West Provinces and Oudh, in the Punjab, in Lower Bengal and Behar, in the Brahmaputra Valley, and in Lower Assam. No official pressure has been brought on the population, and only those have been vaccinated who could be induced to do so by free persuasion. In every locality, efforts were made to apply the operation on parts of large bodies of people living together under identical conditions, in order to compare their resistance in outbreaks of cholera with that of non-inoculated people belonging to the same unit of population. This object has been obtained in 64 British and native regiments, in 9 gaols, in 45 tea-estates, in the fixed agricultural population of the villages parallel to Hardwâr pilgrim road, in the *bustees* of Calcutta, in a certain number of boarding-schools, where the parents agreed to the inoculation of their children, in orphanages, etc. The vast majority of inoculated people lived thus under direct observation of the sanitary and medical authorities of India." (Haffkine, Lecture in London. *British Medical Journal*, 21st Dec. 1895.)

Altogether, upwards of 70,000 injections on 42,179 people—*without having to record a single instance of mishap or accident of any description produced by our vaccines.* Consider the colossal difficulties of this new treatment: the frequent running short of the vaccine, preventing a second injection; the absolute necessity, at first, of using very small doses of a weak vaccine,

lest one disaster should occur; the impossibility of avoiding, now and again, some loss of strength in the vaccine; the impossibility of knowing how long the protection would last. Surely in all science there is nothing to beat this first voyage of adventure single-handed to fight the cholera in India.

Later than Haffkine's 1895 report, we have Dr. Simpson's 1896 report: "*Two Years of Anti-choleraic Inoculations in Calcutta.* By W. J. Simpson, M.D., M.R.C.P., D.P.H., Health Officer, Calcutta." The date of this report is 8th July 1896; and it gives not only the Calcutta results, but all that are of any use for exact judgment:[1]—

"The results of Calcutta are fully confirmed by those obtained in other parts of India, wherever it was possible to make all the necessary observations with precision, and wherever the cases were sufficiently numerous to show the effect of the inoculation.

"Outside Calcutta, since the commencement of the inoculations in India in April 1893, opportunities for an exact comparison of the respective powers of resistance against cholera of inoculated and non-inoculated persons presented themselves; (1) in Lucknow, in the East Lancashire Regiment; (2) in Gaya, in the jail; (3) in Cachar, among the tea-garden coolies; (4) in Margherita, among coolies of the Assam-Burmah Railway Survey; (5) in Durbhanga, in the jail; (6) in the coolie camp at Bilaspur; (7) in Serampur, among the general population."

Here, then, in this 1896 report, are all the results that give an answer to the question, What will happen

[1] For a summary of this report, see the *Lancet*, 8th August 1896. For more recent results, see Surgeon-Captain Vaughan and Assistant-Surgeon Mukerji, in the thirtieth annual report of the Sanitary Commissioner for Bengal (1897). Also the note published by Surgeon-Captain Nott, in the *Indian Medical Gazette*, May 1898.

when cholera breaks out among a number of people living under the same conditions, of whom some have received preventive treatment, and the rest have been left to Nature?

1. *Calcutta* (1894–1896)

"The number of people inoculated during the period under review was 7690; of these, 5853 are Hindus, 1476 Mahomedans, and 361 other classes. . . . Considering that the system is a new one, that the inoculations are purely voluntary, and everything connected with them has to be explained before the confidence of the people can be obtained, and considering how long new ideas are in taking root among the general population—and in this case it is not merely the acceptance of an idea, but such faith in it as to consent to submit to an operation—the number is certainly satisfactory for a beginning. The present problem can be compared with the introduction of vaccination against small-pox into Calcutta. It took 25 years before the number of vaccinations reached an average of 2000; whereas the inoculations against cholera have in two years nearly doubled that average. This is a proof that, in spite of the difficulties which every new movement naturally has to meet with, there are large numbers of people anxious to avail themselves of the protective effect of the inoculations.

"Although all sorts and conditions of individuals, weak and strong, sickly and healthy, young and old, well nourished and badly nourished, and often persons suffering from chronic diseases, have been inoculated, in every instance, without exception, the inoculations have proved perfectly harmless.

"The investigations on the effect of the inoculation are made exclusively in those houses in which cholera has actually occurred, the object being to ascertain and compare the incidence of cholera on the inoculated and not inoculated in those houses in which inoculations

had been previously carried out. *For this purpose, affected houses in which inoculations have not been performed, and inoculated houses in which cholera has not appeared, are excluded.*"

Nature gave a demonstration in 77 houses. In one house, and one only, all the household had been inoculated; in 76, inoculated and non-inoculated were living together; but of these 76 houses, 6 are excluded from the table of results, because the inoculated in them were so few—less than one-tenth of the household—that their escape from cholera might be called chance. The cholera came, and left behind it this fact:—

654 uninoculated individuals had 71 deaths
= 10.86 per cent.
402 inoculated in the same households had
12 deaths = 2.99 per cent.

If we add the 6 houses which Dr. Simpson excludes, we find that in 77 houses there were 89 deaths from cholera, 77 being among the uninoculated and 12 among the inoculated.

Moreover, of these 12 deaths, 5 occurred during the first five days after inoculation—that is to say, during the period in which the protective influence of the vaccine was still incomplete. *Then came a period of more than a year, during which the uninoculated had 42 deaths, and the inoculated had one death.* The remaining 6 of the 12 deaths occurred more than a year after inoculation, and 5 of these 6 had received only one inoculation of the weak vaccine that was used early in 1894.

Take a good instance that came at the very beginning of the work:—

"A local epidemic took place around two tanks in

Kattal Began *bustee*, ward 19, occupied by about 200 people. In this *bustee*, about the end of March, 2 fatal cases of cholera and 2 cases of choleraic diarrhœa occurred. The outbreak led to the inoculation of 116 persons in the *bustee* out of the 200. Since then, 9 cases of cholera, of which 7 were fatal, and 1 case of choleraic diarrhœa have appeared in the *bustee*, and it is a very extraordinary fact that all these 10 cases of cholera have occurred exclusively among the uninoculated portion of the inhabitants, which, as stated, forms the minority in the *bustee;* while none of the inoculated have been affected." (*Cholera in Calcutta in* 1894. W. J. Simpson.)

2. *Lucknow* (1893)

The story of the outbreak of cholera in the East Lancashire Regiment must be read carefully:—

"Rumour magnified the events connected with this outbreak, and distorted the facts connected with the inoculations; and as a result, the current of public opinion, which had previously been in favour of inoculation, set in strongly in the opposite direction. The advocates of anti-choleraic inoculations were abused in no particularly measured terms, and the inoculations were held up to be the source of every possible evil and danger . . . of the most loathsome diseases, and of every ill which man is heir to. The distrust engendered by these misrepresentations and fulminations was, however, only of a temporary nature; and when the exact circumstances came to be known and understood, the confidence created by the Calcutta experience began to be considerably restored. Inoculations were performed in May 1893, in the East Lancashire, Royal Irish, 16th Lancers, 7th Bengal Infantry, 7th Bengal Cavalry, and general populations in the Civil Lines. In 1894, cholera appeared among the native population of Lucknow, in the form of an epidemic distinguished by its extreme viru-

lence, patients succumbing in the course of a few hours. It is stated that the epidemic was of a most malignant type. In the latter part of July it entered the cantonments, and attacked the East Lancashire, almost exclusively confining its ravages to that regiment."

In the East Lancashire, 185 men were inoculated in May 1893. From the statistical returns obtained from the military authorities at Lucknow, it appears that at the time of the outbreak, in July 1894, the strength of the men, including those in hospital, was 773; and of these, 133 had been inoculated, as recorded in the inoculation register, and 640 had not been inoculated.

The following table shows the total number of attacks and deaths in not inoculated and inoculated :—

	Attacks.	Deaths.
	Per cent.	Per cent.
Non-inoculated, 640 . .	120 = 18.75	79 = 12.34
Inoculated, 133 . . .	18 = 13.53	13 = 9.7

The men were moved into camp; but this movement seemed only to make things worse: "the epidemic in the camp appears to have been twice as severe as in the cantonment."[1]

Lucknow came so early in the work of inoculation, that weak vaccines were used in small doses. The cholera, when it broke out, was "of a most malignant type, senior medical officers of long experience in the country stating that such a virulent cholera had not been seen by them for very many years past." More

[1] "The moving into camp, notwithstanding this example, is all the same an excellent measure of defence, and would with reason be adopted in every outbreak." (Simpson, *loc. cit.*)

than a year had elapsed between the inoculations and the outbreak of the cholera. It is no wonder that the regiment was not well protected :—

"The small amount of protection which the inoculations afforded in this case may have depended on the mild effects which the injections produced on the men at the time of the operation in 1893, in comparison with the severity of the epidemic which attacked the regiment. It is recorded in the Lucknow Inoculation Registers that only in two men, out of the 185 inoculated in 1893, a marked febrile reaction was obtained; in 77 individuals the vaccinal fever was only slight, while in 66 there was no reaction: an effect which was due to the weakness of the vaccines procurable at that period of work, and to the small doses used. The influence of the vaccines was possibly further reduced, at the time of the epidemic, by a lapse of fourteen to fifteen months." (Haffkine, 1895 Report.)

3. *Gaya Jail*

On 9th July 1894, an outbreak of cholera occurred in the Gaya jail, and by 18th June there had been 6 cases and 5 deaths. On that day and the next day, 215 prisoners were inoculated. The average number of the prisoners during the outbreak was 207 inoculated, and 202 not inoculated. Surgeon-Major Macrae, superintendent of the jail, reports :—

"The inoculations being purely voluntary, no selection of prisoners was possible, but all classes of the jail were represented—male and female, old and young, habituals and less frequent offenders, strong and weakly, convalescent and even hospital patients sent their representatives; no difference of any kind was made between inoculated and non-inoculated; they were under

absolutely identical conditions as regards food, water, accommodation, etc., in fact in every possible respect."

Of course, the best results could hardly be obtained, because the cholera was already at work: it took about ten days for the 1894 vaccine to produce its full effect; and two inoculations were generally made, one five days after the other. This gradual action of the vaccine is well shown in Dr. Simpson's table:—

	Non-Inoculated, 202.		Inoculated, 207.	
	Cases.	Deaths.	Cases.	Deaths.
During 5 days after 1st inoculation . .	7	5	5	4
During 3 days after 2nd inoculation . .	5	3	3	1
After 3 days after 2nd inoculation . .	8	2	0	0
Total . .	20	10	8	5

Haffkine's comment on these figures must be noted here:—

"In the Gaya jail, the inoculations were *for the first time* applied *in a prevalent epidemic*, and *very weak doses* of a relatively weak vaccine were used. . . . Far higher results have been obtained by an application of stronger doses. In the *bustees* situated round the tanks in Calcutta, where cholera exists in a permanent state, the disease occurred in 36 houses with inoculated people. In each of these houses there was one part of the family inoculated and another not. The observations were continued for 459 days, with the following results:—

During the first period of 5 days, subsequent to the

inoculation with first vaccine, cholera occurred in 8 houses.

75 non-inoculated had 5 cases, with 3 deaths.
52 inoculated had 3 cases, with 3 deaths.

During the second period of 5 days, subsequent to the second inoculation, cholera occurred in 2 houses.

8 non-inoculated had 2 cases, with 2 deaths.
17 inoculated had no cases.

After the 10 *days necessary for the preventive treatment had expired, and up to the* 459*th day, the disease visited* 26 *houses.*

263 *non-inoculated had* 38 *cases, with* 34 *deaths.*
137 *inoculated had* 1 *case, with* 1 *death, in a child that had not been brought up for the second inoculation.*"

4. *Assam-Burmah Railway*

For a good instance of lives saved even during an outbreak, take the Assam-Burmah Railway coolies :—

"Three hundred and fifty[1] Khassia Hill coolies had been collected for the survey party of the Assam-Burmah Railway, and put under the escort of a detachment of Goorkhas, when cholera broke out amongst them. The largest part of the coolies immediately submitted to the preventive inoculation, the rest remained uninoculated. The result was that *among the not-inoculated minority there were* 34 *cases, with* 30 *deaths; whereas the inoculated had* 4 *fatal cases.*" (Haffkine, 1895, Lecture in London.)

5. *Durbhanga Jail* (1896)

The figures in this instance are small : but Surgeon-Captain E. Harold Brown's report is very pleasant

[1] The exact number is 355, of whom 196 were inoculated ; the coolies numbered 343, and the Goorkhas 12. (See Dr. Simpson's 1896 Report.)

reading. Cholera broke out in the jail on 31st March 1896, and by 9th April there had been 8 cases. Next day, 172 prisoners were moved into camp 12 miles away; and 53 were left behind, the sick in the jail hospital, the patients in the cholera huts, with their attendants, the old and infirm, and a few cooks and sweepers. That day, 3 cases occurred in the camp, and 1 in the jail; and on the 11th, at 2 and 4 A.M., 2 more cases were reported in camp. At 7.30 A.M., Haffkine and Dr. Green came to the camp:—

"The prisoners were spoken to on the subject, and seemed to be pleased with the idea, the word *tika* (inoculation), which was familiar to them from its association with small-pox, appearing to appeal to them. They were accordingly arranged in four rows facing the tent, in front of which Dr. Haffkine was about to commence operations. I was the first subject to be inoculated; and after me the jailor, assistant jailor, hospital assistant, and three warders. The first prisoner in the front rank was next brought up and submitted cheerfully; after which, every alternate man was taken, so that no selection of cases was made, until one-half of the total number were inoculated. Those who had not been inoculated were far from pleased at having been passed over; and, to our surprise, they rose almost to a man, and begged to be inoculated; nor were they satisfied when told that the medicine was exhausted."

The dose administered on this occasion (11th April 1896) was stronger than the Gaya jail dose (18th July 1894): it acted in a few hours, and the reaction was well marked.

"There were fresh cases of cholera that day at 12 (noon), 6, 7, and 7.30 P.M., and at midnight, all in those who had not been inoculated, and all terminating

fatally, despite the greatest care and the most prompt and assiduous treatment. On the 12th two further cases occurred, both among the uninoculated, and both died; there being thus eight cases in succession, all from the men who were not inoculated, and all proving fatal."

The inoculations were made at 7.30 A.M. Surgeon-Captain Brown had pain within half-an-hour, and fever in three hours, with temperature 104°, *but this was probably due to the fact that I was not able to rest.* The prisoners, of course, went to bed: they all reacted before 4 P.M., but did not have so much trouble over it. The last case was on the 15th. The outbreak was a bad type of cholera; out of 30 cases 24 died, some of them in 1½ to 4 hours. "To summarise the combined results of the camp and the jail, we find that of a daily average of 99 non-inoculated there were 11 cases, all fatal = 11.11 per cent.; of 110 inoculated there were 5 cases, with 3 deaths = 2.73 per cent."

6. *Bilaspur and Serampur*

Here again the figures are small, but worth noting. In a coolie camp at Bilaspur (Central Provinces) 100 non-inoculated had 5 deaths, and 150 inoculated had 1 death. In Serampur, among the general population, 51 non-inoculated had 5 cases and 3 deaths, and 42 inoculated had 2 cases and 1 death.

7. *The Cachar Tea-Gardens* (1895)

This series of inoculations was begun in February 1895, for the protection of the coolies on various tea-estates. The results are excellent, and deal with large

numbers.[1] The latest report from Dr. Arthur Powell, the Medical Officer, is quoted in Dr. Simpson's 1896 report:—

At Kalain—

1079 not inoculated had 50 cases, with 30 deaths.
1250 inoculated—3 cases, with 2 deaths.[2]

At Kalaincherra—

685 not inoculated had 10 cases, with 7 deaths.
155 inoculated—no cases.

At Degubber—

254 not inoculated had 12 cases, with 10 deaths.
407 inoculated—5 cases, all recovered.

At Duna—

121 not inoculated had 4 cases, with 2 deaths.
29 inoculated—no cases.

At Sandura—

454 not inoculated had 2 cases, with 1 death.
51 inoculated—2 cases, with 1 death.

At Karkuri—

198 not inoculated had 15 cases, with 9 deaths.
443 inoculated—3 cases, with 1 death.

At Craig Park—

185 not inoculated had 1 fatal case.
46 inoculated—no cases.

TOTAL.

Not inoculated, 2976, with 94 cases and 60 deaths.
Inoculated, 2381, with 13 cases and 4 deaths.

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[1] "As a field for testing the value of inoculation, the tea-factories of India possess many advantages. The labourers being under contract, the after-history of those inoculated is easily followed up. Each morning the adults are paraded for roll-call; and all sick must attend hospital, where a record is made of their disease and treatment." (Dr. Powell, *Lancet*, 13th July 1896.)

[2] "It is unfortunate that neither of the fatal cases among the inoculated was seen by any medical man, not even an unqualified doctor Babu." Dr. Powell does not think, from what was told him, that one of them was cholera.

To the preceding instances, which are rather old now, must be added the following more recent report, from the *Indian Medical Gazette*, September 1901 :—

"We are glad to see, from a paragraph in the Report of the Sanitary Commissioner for Bengal (Major H. J. Dyson, I.M.S., F.R.C.S.), that an increased number of anti-cholera inoculations were performed during the year 1900. Assistant-Surgeon G. C. Mukerjee, who was in charge of this work, reports that in the Puralia Coolie Depot no less than 13,291 persons were inoculated against cholera, including over 1000 children. All these cases of inoculation were among labour emigrants proceeding to the tea-gardens of Assam and Cachar. The employers of labour are beginning to realise the value of cholera inoculation. It is unfortunately not always easy, or even possible, to follow up the after-history of persons inoculated; but Major Dyson has quoted a table, received from the Superintendent of Emigration, which shows the number of cases among the inoculated and the non-inoculated at Goalundo. From this table, it is seen that out of 1527 non-inoculated coolies, who passed through Goalundo, 33, or 2.09 per cent., got cholera; whereas of 873 inoculated coolies, only 2, or 0.2 per cent., were attacked by the disease; that is, the unprotected suffered about ten times as much as the inoculated. Assistant-Surgeon Mukerjee also reports that during his cold-weather tour he passed through some villages in the Manbhum district, in which he had practised inoculation the previous year: and, though there had been epidemics of cholera in them, the inoculated persons escaped. They came to him in numbers, stating that they owed their safety to the inoculation."

Of course, the preventive treatment touches points only here and there on the map of India, with its 300,000,000 people. Probably it will never become so general in India as vaccination. Cholera in India

recalls what Ambroise Paré, more than 400 years ago, wrote of the plague, "Here in Paris it is always with us." But, wherever preventive inoculation has been done, there it has done good.

The *Medical Annual* for 1905 contains an account of some preventive inoculations recently made during an epidemic in Japan. Among the inoculated, the attack-rate was much lower than among the uninoculated; and the mortality was 45.5 per cent., as against 75 per cent.

Another most important result of the discovery of the cholera bacillus is its use in diagnosis. For example, if a case of suspected cholera is landed at a British port, the sanitary authority at once takes steps to ascertain whether the specific microbe is present; and, according to the answer given by bacteriology, either allows the patient to proceed on his journey, or adopts measures of isolation to prevent the spread of the disease to others. Thus, thanks to the insular position of Great Britain, this dreadful disease has for many years been prevented from invading her population.

VIII

PLAGUE

THE *bacillus pestis* was discovered by Kitasato and Yersin, working independently, in 1894. Yersin's discovery was made at Hong Kong, whither the French Government had sent him to study plague: an excellent account of his work is given in the *Annales de l'Institut Pasteur*, September 1894. The first experiments in preventive inoculation, in animals, were made by Yersin, Calmette, and Borrel, working conjointly, in 1895. They found that it was possible to confer on animals a certain degree of immunity, by the hypodermic injection of dead cultures of the bacillus. These experiments were made on rabbits and guinea-pigs.

Haffkine's fluid was first used on man in January 1897. It is a *bouillon* containing no living bacilli, and nothing offensive to the religious beliefs of India.[1] He proved its efficacy on rabbits; and then, on 10th January 1897, inoculated himself with a large dose, four times as strong as the subsequent standard dose.

[1] It is said that the Jains object to inoculations on the grounds of religion; and one or two witnesses before the Plague Commission gave evidence to the same effect. But, at Bombay, the high-priest of a great religious community addressed a meeting of 5000 in favour of the new treatment; and the rush of suppliants for inoculation at Hubli and Gaday proves that there is no real religious difficulty. Doctors have been assaulted, as at Poona, so at Oporto; in neither case can we say *Tantum relligio potuit suadere malorum.*

A few days later, Lieut.-Col. Hatch, Principal of the Grant Medical College, Bombay, and other members of the College Staff, were inoculated. These first inoculations were described by Haffkine in a lecture (1901) at Poona:—

"In a short time, a number of the most authoritative physicians in Bombay, European and native, official medical officers and private practitioners, submitted themselves for inoculation. It is a matter of gratification to me to be able to quote, among these authorities, the Head of the Medical Service of the Presidency, Surgeon-General Bainbridge, who not only got himself inoculated, but inoculated also the members of his family. Previous to that, Surgeon-General Harvey, the able Director-General of the Indian Medical Service, submitted himself to inoculation in 1893 against cholera; and, in 1898, against plague. It was the example of these gentlemen, whose competence in the matter of health could not be disputed, that encouraged thousands of people, rich and poor, in Bombay and elsewhere, to come forward for inoculation. Thus his Excellency the Viceroy thought it right to tell you here, in Poona, that previous to his starting for the plague-stricken districts he and his staff had also undergone the prophylactic inoculation. In due course, mothers brought their children to be protected by the new 'vaccination.'"

Within a few months, 8142 persons in or near Bombay were inoculated. It was not possible, in Bombay, during the rush of plague-work, to follow up every one of these 8142 persons. But there is reason to believe, making some allowance for oversights, that only 18 = 0.2 per cent. of them, were attacked during the epidemic; that, of these 18, only 2 died: and that these 2 died within twenty-four hours of inoculation, *i.e.*, had the plague in them already at the time of inoculation.

And, with regard to a small group of the inoculated, there are the following more definite facts. This group lived outside Bombay, across the harbour, in a village called Mora. The population of Mora, at the time of the epidemic, was estimated at less than 1000. Out of this number 429 were inoculated; which, if the population be reckoned at 1000 exactly, left 571 uninoculated. Among the 429 inoculated, there were 7 cases of plague, with no deaths: among the uninoculated there were 26 cases, with 24 deaths.

Just a week after Haffkine had informed the Indian Government that he had tested his fluid on himself, plague broke out in the Byculla House of Correction, Bombay, on 23rd January 1897. Between the 23rd and the afternoon of the 30th, there were 14 cases, with 7 deaths. On the afternoon of the 30th, 152 prisoners were inoculated, and 172 were left uninoculated. The outbreak ceased on 7th February. The figures, as corrected by the Plague Commission, are, among the inoculated, 1 case, which recovered; among the uninoculated, 7 cases, with 2 deaths.

For a full and severe examination of the reports, statistics, and other evidence concerning this and other outbreaks in which preventive inoculations were made, the Report (1901) of the Indian Plague Commission must be studied. The Commissioners, Professor T. R. Fraser, Mr. J. P. Hewett, Professor (now Sir) A. E. Wright, Mr. A. Cumine, Dr. Ruffer, and Mr. C. J. Hallifax, Secretary, travelled and took evidence in India from November 1898 to March 1899: during which time they held 70 sittings and examined 260 witnesses, some at great length. The evidence and the report are published in five large volumes. The report, 540 pages in all, deals exhaustively with the whole subject.

It represents the very least—what might almost be called the very worst—that can be said of Haffkine's fluid: and, of course, it reads rather differently from the reports of the men who, with their lives in their hands, and worked almost past endurance, fought plague themselves. The following paragraphs give, so far as possible, the bare facts of various outbreaks of the disease in 1897–99, in which Haffkine's fluid was used.

1. *Daman*

Plague broke out in Daman, a town in Portuguese territory, north of Bombay, and in constant communication with Bombay by sea, in March 1897. By the end of the month, when a Government cordon was placed round the town, about 2000 out of 10,900 had fled. The outbreak reached its height in mid-April, and was practically over by the end of May. Inoculations were begun on 26th March. The total population on that day (2000 having gone out, and 670 having died of plague) is estimated at 8230. Of these, 2197 were inoculated, and 6033 were left uninoculated. Among the inoculated there were 36 deaths = 1.6 per cent.; among the uninoculated 1482 deaths = 24.6 per cent.

The Commissioners criticise these figures severely, and do not accept them as exact. But they admit the evidence as to the results of inoculation among the Parsee community of Daman. Of this community, 306 in number, 277 were inoculated, and only 29 were left uninoculated. Among the inoculated there was 1 death = 0.36 per cent.: among the uninoculated there were 4 deaths = 13.8 per cent.

They admit, also, the house-to-house investigations

made by Major Lyons, I.M.S., President of the Bombay Government Plague Committee. At the end of May, he visited 89 houses, in 62 of which both inoculated and uninoculated were living together. He found that out of 382 inoculated, 36 had died = 9.4 per cent.; out of 123 uninoculated, 38 had died = 30.9 per cent.

2. *Lanauli*

Plague attacked Lanauli, a small hill-station and railway depot, during April to September 1897. The entire population was estimated at about 2000. Inoculations were begun on 24th July in two wards of the town, and a daily house-to-house inspection was instituted. The figures reported, on the basis of the average daily strength of the two groups, are as follows:—

Inoculated, 323, with 14 cases, of which 7 died = 2 per cent.
Uninoculated, 377, with 78 cases, of which 57 died = 15 per cent.

The Commissioners criticise the method on which these figures are based, and do not accept them as accurate. But they agree that inoculation "exerted a distinct preventive effect"; and they admit Major Baker's evidence—"In the place where inoculation had been made use of, the town was thriving and full of people; and the other part of the town was absolutely empty. One side had plague, and the other had none."

3. *Kirki*

The figures here were obtained under especially favourable circumstances; and the Commissioners have,

practically, no fault to find with their accuracy. The following account is by Surgeon-Major Bannerman, Superintendent of the Plague Research Laboratory, Bombay:—

"Plague broke out in Kirki, in the artillery cantonment, situated four miles from Poona; and the followers of the four batteries stationed there suffered severely. These men were living with their families in lines on a sloping plain, under military discipline, and in circumstances far superior in a sanitary sense to those of the average villager. When the disease appeared, the lines were isolated, so that none could enter or leave without the knowledge of the military. A special hospital was erected close by, where all sick persons were sent as they were discovered by the search parties of European artillerymen, who visited each house thrice daily. It is therefore probable that all cases of plague were promptly discovered and removed to hospital: and in each case the usual disinfection was thoroughly and systematically carried out. Yet, in spite of all this, it was found that, in those not protected by inoculation, 1 out of every 6 of the population was attacked, and 2 out of every 3 attacked died. The epidemic was, therefore, a severe one. The population of the lines numbered 1530; and, out of these, 671 volunteered for inoculation. At the close of the epidemic, the plague-hospital admission and discharge book was examined, and compared with the register of those inoculated, when the following result was got. The population operated on being under military discipline, and confined to their lines, makes the accuracy of the figures undoubted:—

Inoculated, 671, with 32 cases, of which 17 died = 2.5 per cent.
Uninoculated, 859, with 143 cases, of which 98 died = 11.4 per cent.

"Here, then, is seen a body of people divided into

two groups by the fact that one had undergone inoculation and the other not, *but differing in no other way*, reacting towards plague in such a markedly different manner that the conclusion is forced on one, that the inoculation must be the cause. Seeing the absolute similarity of conditions, *the* 671 *inoculated should have had proportionately* 112 *cases and* 77 *deaths, if they had remained as susceptible to the disease as their uninoculated brothers, sisters, parents, wives, husbands, children; but, instead of that, they had only* 32 *cases and* 17 *deaths.* This death-rate would doubtless have been still further reduced, but for the fact that a very much weakened vaccine had to be used, owing to the demand having got beyond the resources of the laboratory at that time."

4. *Belgaum*

In Belgaum, a town of Southern India with a normal population of about 30,700, two outbreaks of plague occurred in quick succession. The first outbreak lasted from November 1897 to May 1898; the second, from July 1898 to January 1899. During the two epidemics, 2466 persons were inoculated. Of these, it was reported that only 61 (or 62) had been attacked, of whom 33 died = 1.34 per cent. But these figures, in the judgment of the Commission, cannot be accepted as even approximately correct. There are, however, two groups of these Belgaum cases, one of which the Commission admits as substantially accurate, and the other as absolutely accurate. These groups are, (1) the Army cases; (2) the cases reported by Major Forman, R.A.M.C., Senior Medical Officer of the Station.

(1) *The Army Cases.*—These cases occurred in the 26th Madras Infantry, which was living in lines close to the cantonment and the city. The first case of

plague in the regiment was on 12th November 1897. Ten days later, the regiment was moved out into camp. Inoculation was begun, by Surgeon-Major Bannerman, on 23rd December, up to which time there had been, among the regiment and its families and followers, 78 cases, with 49 deaths. The following account of the inoculations is given by Surgeon-Major Bannerman:—

"No difficulty was experienced in persuading the men to consent to inoculation, when it was explained to them that they would be free to return to their houses in the lines after being operated on. General Rolland was the first to be operated on, and his example, combined with that of the officer commanding, and their medical officer, who were all operated on in front of the men, sufficed to convince the Sepoys of the harmlessness of the operation: and the only difficulty that then remained was to perform the operation fast enough. . . . The community was, practically, completely inoculated by the end of the year. The total operated on was 1665, out of a population of 1746 living in the lines at that date. The 81 not operated on were infants, women far advanced in pregnancy, and the sick in hospital chiefly, though one solitary Sepoy has, up to the present time, refused to submit to operation."

From this time onward to the end of the first epidemic, though the disease was at its height in January in the neighbouring city and cantonment, and though the men were allowed to go freely to these places after inoculation, *only 2 out of the 1665 were attacked, and both recovered.*

When the second epidemic came, in July 1898, the troops, families, and followers, were reinoculated at their own request, 1801 in all. "Practically no one was left in the lines unprotected by inoculation." From this time onward to the end of the second epidemic, though

it was much more severe than the first, only 12 cases occurred. *In the first epidemic, before inoculation, 78 cases occurred, and 2 after it: in the second, and much more severe, epidemic, though the sanitary measures adopted in both epidemics were similar, only 12 cases occurred.* "It would hardly appear to be open to doubt," says the Commission, "that the practical immunity of the regiment, during the second outbreak, was due to inoculation."

(2) Major Forman's evidence before the Commission is very striking, though the figures are small. The following abstract of it is given in the Report of the Commission :—

"The groups of persons, concerning whom Major Forman gave us evidence, were his private servants, and the hospital attendants of the Belgaum Station Hospital with their wives and children. He inoculated these groups when plague first broke out in the town, and was able to keep in touch with them continuously after that time. Regarding the first group, he says, bringing down their history to 3rd March 1899, 'Of my private servants there were in all, including their wives and children, 28 people inoculated. There have been no cases of plague, and no deaths up to date. There were 3 uninoculated. One was a child of 9 years of age, whose father refused to allow it to be inoculated. It died of plague 12 days after the other people were inoculated. The other 2 cases that were not inoculated were not so distinctly under my own observation. One was a sweeper employed in the cantonment, and sleeping in my compound: he, I am told, died of plague some months afterwards. The other was my water-carrier: he threw himself into a well: I was informed that he had buboes and fever, and ran away to escape segregation. Of the 28 inoculated, none died of plague: and of 3 unin-

oculated, 2 are said to have died of plague, and 1 undoubtedly died of plague.'"

"Regarding the second group of which he gave us particulars, Major Forman said that, out of 90 hospital servants, 87 were inoculated. Of the inoculated persons, 1 died from fever and endocarditis, and 1 died of plague. Excepting these two, the rest of the inoculated were alive and well in March 1899. Only 3 persons remained uninoculated. Of these, one was not operated upon, because she had recently been delivered; another was not operated upon, because she was pregnant; and the third was a boy of 16 years of age, whose father refused to let him be inoculated. The boy died of plague, two months after the inoculation of the rest of the hospital servants had been done. One of the two uninoculated women died of plague two days after the boy, she having been in attendance upon him. The other uninoculated woman remained well."

5. *The Umarkhadi Jail, Bombay*

Plague broke out in this jail on the last day of 1897, and 3 prisoners died. Next day, 1st January 1898, all the prisoners were paraded, and all were willing to be inoculated. But it was decided to divide them into two equal groups, and inoculate one group. There were 402 altogether: 2, when their turn came, refused to be inoculated: thus 199 were inoculated, and 203 were left uninoculated. No distinction was made between the two groups: "They had the same food and drink, the same hours of work and rest, and the same accommodation." The plague did not come wholly to an end till March. The figures, since the inmates of a jail are a shifting population, are based on

the average daily number of each group : this was 147 for the inoculated, and 127 for the uninoculated. The figures are :—

Average Daily Number.		Cases.	Deaths.
Inoculated . .	147 .	3	0
Uninoculated .	127 .	9	5

The Commission draw attention to "the important fact that, during the whole period of the outbreak, the number of attacks among the inoculated was only one-third of the number among the uninoculated ; and that the disease among the inoculated was remarkably mild, resembling mumps more than plague, though the cases among the uninoculated were of average severity." According to Surgeon-Major Bannerman, the hospital authorities were doubtful whether these three cases among the inoculated were plague at all.

6. *Undhera*

The figures for Undhera are very valuable : "The conditions," says Surgeon-Major Bannerman, "approached very nearly the strictness of a laboratory experiment." Even the Commissioners are enthusiastic here.

Undhera is an agricultural village, 6 miles from Baroda. Plague broke out in it, in January 1898. A careful census was taken, and showed a population of 1029. By 12th February there had been 76 deaths. On that day the village was visited by Mr. Haffkine,

Surgeon-Major Bannerman, and other experts, and 513 persons were inoculated :—*By reference to the census papers, the whole of the inhabitants were called out, house by house, and the half of each household inoculated. In this way, an endeavour was made to inoculate half the men, half the women, and half the children in each family, and to arrange that a fairly equal proportion of the sickly-looking should be placed in each division.* The plague lasted 42 days after the inoculations, and affected 28 families. On 4th April a house-to-house investigation was made by Mr. Haffkine, Surgeon-General Harvey, Surgeon-Major Bannerman, and Captain Dyson. The figures are as follows :—

Population on 12th February.		Cases.	Deaths	Mortality.
1029 – 76 = 953	Inoculated, 513	8	3	0.6 per cent.
	Uninoculated, 440	28	27	6.0 per cent.

Thus, out of 28 families, where the protected and the unprotected lived and ate and slept together, the protected, 71, had 3 deaths ; and the unprotected, 64, had 27. The percentage of attacks was four times higher among the unprotected ; the percentage of deaths was ten times higher.

7. *Khoja Community, Bombay*

The head of this community, H.H. Sir Sultan Shah, Aga Khan, K.C.I.E., opened a private station for the inoculation of the community in March 1897, and again in December of that year. He was himself inoculated

three times, and many of the community so often as five times. The work of inoculation went on daily, and by 20th April 1898 the number of persons inoculated or reinoculated was 5184. The whole community, according to a careful census taken at the beginning of 1898, numbered 9350; but, since many families had fled to avoid the infection, this number is too low. The Commissioners guess 9770: Haffkine, to the disadvantage of his own statistics, guesses so high as 13,330. The number of the inoculated or reinoculated shifted, of course, as the work went on: their average daily number during the four months of plague, January to April 1898, was 3814.

During these four months, the number of deaths *from all causes* in the whole community was 184. According to the average mortality of the community in times of no plague, the deaths *from all causes* during four months would be 102. It may fairly be assumed that the extra deaths, 82, were due to plague: and, indeed, 64 plague-deaths were either acknowledged by the relatives, or certified by the burial-books of the community. *Of these 82 deaths, 3 occurred among the inoculated or reinoculated, and 77 among the uninoculated.*

The Commissioners find fault with these figures: "Nevertheless, quite apart from the statistics put before us, which we think inaccurate, we do not doubt that inoculations had a good effect, especially as much weight must be allowed to the opinion of a community so intelligent as that of the Khojas."

8. *Hubli*

This, the greatest and most amazing of all instances of preventive plague-work, was done in a town of 50,000 persons. The following report, by Surgeon-Captain Leumann, was forwarded to the Plague Commissioners by Mr. E. K. Cappel, Collector of Dhárwár, with this comment:—

"The town of Hubli—a mercantile town of over 50,000 inhabitants—was attacked by plague in an epidemic form at the commencement of the monsoon rains. The average rainfall between April and October amounts to more than 28 inches. Under these circumstances, although a large and weather-proof health camp had been prepared for emergencies, complete evacuation of the infected town-site was impossible; and the attempt to effect it would have led to the severest hardships and to the immediate spread of the disease into surrounding villages and districts. It was for this reason that the determination was formed to make a bold and comprehensive experiment with the prophylactic, and not on any *à priori* grounds. If this experiment had failed, the results, judged by the actual mortality among the uninoculated, would have been appalling. *All possible sanitary measures in the shape of disinfection, unroofing of houses, and segregation, were applied concurrently with inoculation, as Government are already aware; but the rate of mortality among those who held back from inoculation rose at one time to a height which, I believe, has never been approached elsewhere.* . . .

"However, the experiment, in the hands of Dr. Leumann, did not fail, and it has afforded a demonstration of success which is of Imperial importance. Many thousands of lives have undoubtedly been saved, and at the present moment the plague mortality is merely sporadic,

and Hubli is steadily regaining its normal population and trade, though surrounded by infected villages."

The Hubli report must be put at full length, for the vivid picture it gives of plague in India, and of the difficulties besetting the magnificent work of the Indian Medical Service. It is a story that Mr. Kipling ought to write. And it is to be noted that Surgeon-Captain Leumann, who saved Hubli, recognised the extreme importance of other methods than inoculation —disinfection, isolation of cases, evacuation of infected districts. He says:—

"While paying the highest tribute to the value of Mr. Haffkine's inoculation method, which I claim, here in Hubli, to have put to perhaps the severest test to which it has yet been subjected, I am of the opinion that individual protection is, on however great a scale conducted, of less importance to that of general protection and hygiene (considering each method separately, that is to say), for it seems to me more radical, if not more rational, to eradicate a disease than to leave it to pursue its course and only protect people against its ravages."

Sanitation, therefore, was Dr. Leumann's faith. Now for his works:—

"I first started inoculation here on 11th May. . . . When I began my inoculations, I operated first of all on some European or native gentlemen in front of a crowd of poor and low-caste people, whom I had gathered together in the worst-affected area, and they were thus soon induced to ask for inoculation themselves. . . . They have presented themselves, by the hundred, at all times of the day, before myself and others, for the purpose of being inoculated.[1] . . . I have never experienced the

[1] Compare the account of the inoculations at Gaday, in the *Lancet*, 11th February 1898: "To see the crowd waiting and

slightest difficulty in inoculating Mussulmanis or any other purdáh women in Hubli. . . . The very men who, in March last, created a disturbance in Hubli, were not only the first and the most willing to undergo inoculation, but also to bring their wives and families to my hospital, or to invite me to their homes to inoculate them.

"Inoculated persons holding certificates of double inoculation have, at my special wish and order, been left in their homes throughout this epidemic; only their clothes, house, and property being disinfected on the occurrence of a plague case or death in their house. As the vast majority of plague cases have never been notified before death in Hubli (nor, in my experience of nearly two years, elsewhere, if native supervision be largely resorted to), it will readily be understood that the majority of the inoculated have actually been living in the same house, or even room, with a plague case (often of the pneumonic type, whose terrible power of spreading the disease was first shown by Professor Childe, I.M.S., of Bombay) during the whole of the time that case was living, probably attending on the patient, breathing the same stuffy air, and, perhaps, sharing the same blanket; and I attach at the end of this report a long series of cases where such conditions have occurred, *the non-inoculated dying of plague, and the inoculated escaping, almost to a man.*

"Various critics on my work, not knowing what the actual facts were and are, have at different times asserted that the inoculated inhabitants of Hubli left the town in larger numbers than the non-inoculated. Exactly the reverse was the case. The British officers on plague duty here, and all the Divisional Superintendents, invariably replied (officially and in writing when so required) that the non-inoculated left Hubli in far greater

struggling to pass the barrier is a strange sight; old men and women, young children, and mothers with babes in their arms, form a daily crowd numbered by hundreds, who wait for hours to get their chance of the day's inoculation."

numbers and proportion than the inoculated; and my own observations entirely bear out this statement.

"It has been urged that those who received inoculation were of a class or classes better protected than others against plague by reason of their habits, the food they eat, the houses they live in, etc. In reply, I unhesitatingly state that if there be but one town in India where that line of argument will not hold good, it certainly is Hubli; for *not only were the poorer, dirtier, lower-caste people the first to be persuaded to receive inoculation, but I made it my personal and special duty to work amongst them.* My first few thousand inoculations were almost entirely amongst the lowest and poorest of the people. The Brahmins are, perhaps, of all castes, supposed to be the most cleanly in their houses, habits, etc., yet the Brahmins of Hubli (who at first, imagining themselves immune, were the foremost and greatest perverters of the truth concerning its efficacy, and the last to apply for the protection inoculation affords), simply inundated the various inoculation centres, as soon as plague began to spread in their midst, clamouring for the very method of which they had only lately tried to prevent others from availing themselves.

"Unfortunately, the average native, educated or not, appears to have the very greatest aversion to notifying any case of sickness—plague or other—and hence, in my opinion, it becomes more necessary than ever to protect the people by inoculation, since they will not help to protect themselves by the foremost and simplest of sanitary and hygienic measures.[1] With so few police (and those

[1] Compare the account given by the Rev. H. Haigh (*Methodist Recorder*, December 1898), of the plague at Bangalore: "The native population do all they can to elude the vigilance of the authorities. In order to escape segregation, the householders in many instances refrain from reporting plague, and not infrequently bury the corpse secretly. Not only is any spare piece of ground used as a burial-place, but the body is at times thrown into a well or tank, or dropped over the wall of some European compound. During one week three plague corpses were found, badly decomposed, in reservoirs commonly resorted to for drinking purposes."

none too good) to help one; an inadequate British Staff; with so much reliance placed in Native Superintendents and Supervisors, and a Municipality so bankrupt that it could not apparently afford to buy enough blankets out of its own funds for the patients in the Plague Hospitals —the work of segregation, house-to-house inspection, etc., became, from a medical point of view, absurdly insufficient.

"The total number of inoculations performed in Hubli, both on actual inhabitants and on people from outside (villages) between 11th May and 27th September, amounts to some 78,000 altogether."

I

Dates.	Census of Hubli.	Non-inoculated.	Inoculated.	Plague-deaths among Non-inoculated.	Plague-deaths among Inoculated.
Five weeks from May 11 to June 14	Fell from 50,000 to 47,427	44,573	2,854	47	1
Week ending June 21	47,082	41,494	5,588	22	3
,, June 28	47,485	39,042	8,443	29	1
,, July 5	46,537	36,020	10,517	55	6
,, July 12	46,518	33,255	13,263	34	6
,, July 19	45,240	29,716	15,524	82	7
,, July 26	43,809	24,112	19,697	100	15
,, Aug. 2	43,707	21,031	22,676	140	16
,, Aug. 9	42,768	15,584	27,184	272	19
,, Aug. 16	40,441	10,685	29,756	386	61
,, Aug. 23	39,400	6,367	33,033	371	41
,, Aug. 30	38,210	4,094	34,116	328	28
,, Sept. 6	38,382	2,731	35,469	227	34
,, Sept. 13	38,408	1,116	37,292	138	47
,, Sept. 20	39,142	937	38,205	106	55
,, Sept. 27	39,315	603	38,712	58	20

II

Dates.	Plague death-rate. Comparison per 1000 between		Percentage reduction of Plague death-rate in favour of the Inoculated.
	Non-inoculated.	Inoculated.	
Five weeks from May 11 to June 14	1.022	.350	Over 65 per cent.
Week ending June 21	.530	.527	About 1 ,,
,, June 28	.742	.118	Nearly 85 ,,
,, July 5	1.524	.570	About 63 ,,
,, July 12	1.022	.452	Nearly 56 ,,
,, July 19	2.793	.450	84 ,,
,, July 26	4.147	.761	82 ,,
,, Aug. 2	6.656	.705	89 ,,
,, Aug. 9	17.325	.698	Over 96 ,,
,, Aug. 16	33.694	2.083	94 ,,
,, Aug. 23	57.011	1.241	98 ,,
,, Aug. 30	80.116	.820	98 ,,
,, Sept. 6	83.112	.958	99 ,,
,, Sept. 13	112.903	1.260	Over 99 ,,
,, Sept. 20	113.127	1.439	Over 99 ,,
,, Sept. 27	96.185	.517	Over 99 ,,

"It appears that if the 47,427 inhabitants had remained, as they did—in their town, without running away by rail or otherwise, or without camping out in a mass—and if no inoculation had been resorted to—they would have lost 24,899 souls, or a little over half of their number. The official records show that this has actually occurred, during the present terrible outbreak, in a number of large villages, of 2000 inhabitants and over, in the Hubli *taluka* and elsewhere in the Dhárwár District, where no inoculation was done, and no camping-out was possible on account of the wet weather." (Haffkine's commentary on Dr. Leumann's report.)

That is the story of Hubli; and, as it stands, it is almost incredible. The Commissioners, by very strict inquiry, reduced it to credibility without robbing it of glory. The inquiry brought out more instances of the immeasurable difficulty of the work. Natives who wished to avoid inoculation would escape through the back door at the sight of a plague officer: bribery,

personation, sale or transfer of certificates of inoculation, concealment of cases and of deaths, were all practised by those who wished not to be inoculated, or to get the privileges of the inoculated without inoculation, or to save their infected houses from being disinfected and unroofed. Again, with the people dying like flies, and many of them bearing no mark of identification, and with the medical officers overworked past human endurance, the wonder is, not that the statistics were faulty, but that there are any statistics at all. Certainly, the Commission is well within the mark in saying, "It is quite clear that a very large number of lives must have been saved in Hubli by inoculations during the whole course of the epidemic there. *Moreover, we may note that an arithmetical estimate is not the only criterion by which we can appreciate the value of inoculations. And in Hubli their value is approved by the consensus of opinions of officers who have seen probably far more of this process and its results in practice than any other persons in India, and who, having every facility for forming a sound judgment as to its effect where plague was really virulent, are satisfied as to its great value.*"

Finally, as at Daman so at Hubli, there are lesser groups of statistics, of that kind which is *approved by the consensus of opinions of officers.* These are, (1) Lieutenant Keelan's house-to-house investigation; (2) the Southern Mahratta Spinning Mills; (3) the Southern Mahratta Railway employés.

1. Lieutenant Keelan made a house-to-house visitation of 200 houses, in each of which there were protected and unprotected persons living together, and in each of which there had been one or more cases of plague. The figures for 69 of these houses are

appended to Captain Leumann's report. They are as follows :—

	Inmates.	Cases.	Deaths.	Mortality.
Inoculated	336	11	4	1.19
Uninoculated . . .	144	84	80	55

These 69 houses were selected: there was nothing unfair in the method of selection, still, they were "good houses"; they are not, therefore, exact for statistics; but, as the Commissioners say, they are "of interest as quite special examples of successful inoculation."

2. In the Southern Mahratta Spinning and Weaving Company's Mills, a careful record of inoculation was kept and checked by the manager. The number of the workpeople at the time when inoculation was begun, 21st June, was 1173. At the end of the epidemic the figures were :—

		Deaths.	Mortality per cent.
Inoculated twice . .	1040	22	2.11
Inoculated once . .	58	8	13.79
Uninoculated . . .	75	20	26.66

Here, again, the figures have not a statistical value: "We are not informed whether the inoculations were performed simultaneously; or at what stage of the outbreak the average strength of the inoculated was reached." All the same, what Major Bannerman says of them is true—*The experience in this company's mill at*

Hubli should be an object lesson to all mill-owners in plague-stricken towns.

3. The figures for the Southern Mahratta Railway are given by Major Bannerman in his "Statistics" (1900): they are not mentioned in the Report of the Plague Commission. They are of great value, because the daily shifting of the numbers was recorded as the work of inoculation went on, and the date of each case of plague was also noted. Major Bannerman gives the following account:—

"The railway employés were living in barracks, and in the railway yard, apart from the general population of Hubli town. *They were under close daily inspection by English officials*, who formed a committee for this purpose, with Dr. Chenai as their medical adviser. The results may therefore be regarded as accurate in a high degree, the numbers dealt with not being excessive, and the supervision strict."

The figures, based on the average numbers in each group, are as follows:—

	Cases.	Deaths.	Mortality per cent.
Twice inoculated . . 990	6	1	0.1
Once inoculated . . 270	5	1	0.3
Uninoculated . . . 760	35	21	2.7

These eight instances must suffice: many must be left out—among them, Dhárwár and Gadag, where Miss Corthorn, M.B., did work as splendid as Leumann's work at Hubli; and Mr. Anderson's work in the Ahmednagar villages; and many more. These plague-reports

are to be read, not for their record of heroic zeal and resourcefulness, but only as one more example of many thousand lives saved by a method learned from experiments on animals.

But, of course, there is not, and perhaps there never will be, a national acceptance and adoption of this method through the length and breadth of India. It does not work miracles; it is an uncomfortable process to submit to; privileges must be offered with it, or the native will often prefer to take his chance; the protection is of uncertain duration; all sorts of lies are told about it, partly by anti-vivisectionist writers, partly by native political agitators, partly by the *hakims*. For instance, at a meeting of *hakims* at Masti, Lahore, on 11th April 1898, the following resolutions were passed:—

"That in the opinion of this meeting the bubonic plague is not a contagious disease. It originates from poisoned air, and this poison is created in the air on account of atmospherical germs and the excess of terrestrial humidities.

"That this meeting, having carefully considered the Resolution of the Punjab Government (11th January 1898), is of opinion that the rules embodied in that Resolution (isolation, disinfection, etc.), are unnecessary under the principles of Unani medical science."

And among statements to be made to the Plague Commissioners was the following, from a native practitioner in Bombay (April 1899):—

"I do not think the plague was imported in Bombay from Hong Kong or anywhere else. I attribute three sources of causes of outbreaks of plague in Bombay:

(*a*) The predisposing cause was the Bombay Municipality; (*b*) The exciting cause was the Nature herself; (*c*) The aggravating cause was the Plague Committee."

All these difficulties were well stated by Surgeon-General Harvey, Director-General of the Indian Medical Service, at the discussion on Haffkine's discourse before the Royal Society, June 1899 :—

"The people of England should consider the difficulties attending the work of a bacteriologist in India. . . . He had no doubt as to the value of the inoculations. At Undhera he carefully examined the results of the experiment, and, as far as he could judge, there was no possibility of error. The results in that experiment were such as to be 90 per cent. in favour of the inoculated against the uninoculated. The natives of India were, however, a strange people, and it was difficult to prophesy how they would act. In Calcutta, the mention of inoculations had driven in hot haste from the city 300,000 people, many of whom afterwards returned and were inoculated; while at Hubli he had seen the inhabitants come in their thousands to be inoculated and pay for the inoculations. The medical officer in charge at Hubli had performed about 80,000 inoculations, and had only observed some 12 abscesses. He thought that 12 abscesses only, in 80,000 inoculations, showed good results. But, after all, what were the numbers of inoculations performed to the 300,000,000 inhabitants of India? He felt that even if every one consented to be inoculated it was impossible to provide the vaccine or the medical officers for such a demand. It was accordingly to sanitary improvements that he looked with the most confidence to protect India against the plague."

Therefore, now and for many years to come, preventive inoculation must fall into line with the other

world-wide ways of fighting plague—quarantine, notification, isolation, all sanitary measures, destruction of rats —*le rat, le génie de la peste*—evacuation of infected towns, disinfection or unroofing of infected houses. Happily, this is just what it does. That admirable paper, the *Indian Medical Gazette* (September 1901), has put this fact very simply: "No one ever imagined that inoculation was the *only* means of fighting plague. Its great value consists in its immediate application. To sanitate, ventilate, and practically rebuild a town or village takes time; and in the meantime thousands die." For sudden outbursts of plague—since rats are one chief source of infection, and notification is fundamentally abhorrent to native custom, and evacuation may ruin trade, or spread infection, or be impossible by reason of the rains—since "East is East, and West is West"—it is not always possible to provide, for an Indian village smitten by plague, the excellent arrangements of the Western world. In all such cases, and in all cases of epidemic plague within narrow limits, as in jails, barracks, mills, and the like centres of human life; and in all inner communities, such as the Parsee community at Daman, or the Jewish community at Aden—by every test of this kind, the saving power of preventive inoculation has been proved, again and again, past all doubt. As for those larger death-traps, Hubli, Dhárwár, and the rest of them, here, though the statistics are inexact, we have the word of the men and women themselves who stood between the dead and the living, and the plague was stayed. Such faults as there were, in 1899, in the treatment—the contamination of this or that stock of the fluid, and the inadequate method of standardisation—have been duly noted by the Commission. The rush for

the fluid in 1899 may be estimated from the following paragraphs:—

(i.) *Paris.* "The preparation of anti-plague serum is being rapidly proceeded with; up to the present time the Institute has supplied it, in response to all the very numerous requests which have come from Portugal, Spain, Italy, and Turkey, without encroaching on the reserve kept in readiness for Paris and the departments." (*Lancet*, 16th September 1899.)

(ii.) *India.* "The spread of plague westward to Spain and Portugal seems to have excited more or less general alarm, and I hear that an unprecedented demand has suddenly arisen for the plague prophylactic fluid. The Government of India have been asked the cost of supplying from 50,000 to 100,000 doses, and the earliest date at which this quantity could be despatched. It is also desired to know if in case of need 50,000 doses a week could be sent to London. Russia desires to obtain a considerable stock for Port Arthur. Italy has been making inquiries for home use; and also Portugal, in order to inoculate at Mozambique. The present laboratory is at Government House, Parel, Bombay, and has only recently been fitted up by the Government of India. About 10,000 doses a day can be turned out, but it is thought that still further enlargements will be required if the demand should increase beyond this amount." (*Lancet*, 23rd September 1899.)

It would take too long for the present purpose to consider what has been done, not only for the prevention of plague, but also for its cure by a serum treatment. The results obtained by this treatment in India have not been very good; but Yersin and others report better results in other countries. Good results are reported from Amoy (1896), Nhatrang (1898), Oporto (1899), and Buenos Ayres (1899–1900). In

Glasgow, the prophylactic use of Yersin's serum seems to have done excellent service: the success of its curative use was not very striking. The curative results at Nhatrang (Yersin, *Annales de l'Institut Pasteur*, March 1899) are notable. Nhatrang is an Annamese fishing-village; and the plague, when it was left to itself, killed every case that it got:—

"La peste s'est montrée excessivement meurtrière chez les Annamites. Sur 72 cas de peste, 39 personnes chez lesquelles la maladie a évolué normalement, ou qui n'ont été traités que par des médecins indigènes, sont mortes sans exception. Les 33 autres cas ont pu être traités par le sérum, quelquefois dans de bonnes conditions, mais le plus souvent quelques heures seulement avant la mort. Malgré cela, nous avons obtenu 19 guérisons et 14 décès, ce qui fait une mortalité de 42 per cent., chez les traités. *Ainsi, d'une part,* 100 *pour* 100 *de mortalité chez les non-traités; de l'autre,* 42 *per cent. chez les malades qui ont reçu du sérum.* Ces chiffres confirment les résultats que j'avais obtenu en Chine en 1896."

A long review of this curative treatment, fairly hopeful but nothing more, is given in the Report of the Plague Commission, vol. v., pp. 269–320. The Commissioners are of opinion that it ought not yet to be extended, as a general measure, over all the districts affected with plague; and that there is need of more work in bacteriology before it can be thus extended. "We desire to record our opinion that, though the method of serum-therapy, as applied to plague, has not been crowned with a therapeutic success in any way comparable to that obtained by the application of the serum method to the treatment of diphtheria, *none the less the method of serum-therapy is in plague, as in other*

infectious diseases, the only method which holds forth a prospect of ultimate success."

It is a strange contrast, between this opinion and the statements made by the opponents of all experiments on animals. Some of these statements will be found in Part IV. of this book. Happily for the world, no amount of foul language can hinder the good work; and, when we talk of *Empire-building*, and of *deeds that win the Empire*, we must reckon bacteriology among them: as Lord Curzon did, in his speech at Calcutta, March 3, 1899—*What is this medical science we bring to you? It is built on the bed-rock of pure irrefutable science; it is a boon which is offered to all, rich and poor, Hindu and Mohammedan, woman and man.*

IX

TYPHOID FEVER. MALTA FEVER

Typhoid Fever

The names of Klebs, Eberth, and Koch, are associated with the discovery, in 1880–81, of the bacillus of enteric fever, *bacillus typhosus;* and it was obtained in pure culture by Gaffky in 1884. It has been studied from every point of view, in man and in animals; in the blood, tissues, and excretions; in earth, air, water, milk, and food; in its distribution, methods of growth, and chemical products. Especially, the study of its chemical products has been directed toward (1) immunisation against the disease, (2) bacteriological diagnosis of the disease at an early stage.

The date of the first protective inoculations against typhoid is July to August 1896: they were made at Netley Hospital, by Professor Wright and Surgeon-Major Semple. The first inoculations in Germany, made by Pfeiffer and Kolle, were published two months later. The story of these famous Netley inoculations is told in the *British Medical Journal*, 30th January 1897. Eighteen men offered themselves—

"A good deal of fever was developed in all cases, and sleep was a good deal disturbed. These constitutional symptoms had to a great extent passed away by the morning, and laboratory work went on without interruption. . . . With two exceptions, all these vaccinations

were performed upon Medical Officers of the Army or Indian Medical Services, or upon Surgeons on Probation who were preparing to enter those services."

Good luck attend all eighteen of them, and immunity against typhoid, wherever they are. The doses that they received were estimated in proportion to the dose that would kill a guinea-pig of 350–400 grammes weight; and the protective fluid contained no living bacilli :—

"The advantages which are associated with the use of such 'dead vaccines' are, first, that there is absolutely no risk of producing actual typhoid fever by our inoculations; secondly, that the vaccines may be handled and distributed through the post without incurring any risk of disseminating the germs of the disease; thirdly, that dead vaccines are probably less subject to undergo alterations in their strength than living vaccines."

The first use of the vaccine during an outbreak of typhoid was in October 1897, at the Kent County Lunatic Asylum. The treatment was offered to any of the working staff who desired it:—

"All the medical staff, and a number of attendants, accepted the offer. *Not one of those vaccinated*—84 *in number—contracted typhoid fever : while of those unvaccinated and living under similar conditions*, 16 *were attacked.* This is a significant fact, though it should in fairness be stated that the water was boiled after a certain date, and other precautions were taken, so that the vaccination cannot be said to be altogether responsible for the immunity. Still, the figures are striking." (*Lancet*, 19th March 1898; see also Dr. Tew's paper, in *Public Health*, April 1898.)

Certainly, they are striking; so is the story of the eight young subalterns on the Khartoum expedition, of

whom six were vaccinated, and two took their chance. The six escaped typhoid, the two were attacked by it, and one died. But these figures are too small to be of much value.

The first anti-typhoid inoculations on a large scale were made among British troops in India (Bangalore, Rawal Pindi, Lucknow), when the Plague Commission, of which Professor Wright was a member, was in India, November 1898 to March 1899. These inoculations were voluntary, at private cost, and without official sanction; though the original proposal for them, in 1897, had come from the Indian Government. Pending official sanction, they were stopped. Then, on 25th May 1899, the Indian Government made application to the Secretary of State for India that they should be sanctioned, and should be made at the public cost. The application is as follows:—

"The annual admissions *per mille* for enteric fever amongst British troops in India have risen from 18.5 in 1890 to 32.4 in 1897, while the death-rate has increased from 4.01 to 9.01; and we are of opinion that every practicable means should be tried to guard against the ravages made by this disease. The anti-typhoid inoculations have been, we believe, on a sufficiently large scale to show the actual value of the treatment, while the results appear to afford satisfactory proof that the inoculations, when properly carried out, afford an immunity equal to or greater than that obtained by a person who has undergone an attack of the disease; further, the operation is one which does not cause any risk to health. In these circumstances, we are very strongly of opinion that a more extended trial should be made of the treatment; and we trust that your Lordship will permit us to approve the inoculation, at the public expense, of all British officers and soldiers who may voluntarily submit themselves to the operation."

On 1st August, the Secretary of State for India announced in Parliament that this treatment, at the public expense, had been sanctioned.

On 20th January 1900, Professor Wright published in the *British Medical Journal* an account of these 1898–99 inoculations in India. "They were undertaken under conditions which were very far from ideal. In particular, there is reason to suppose that the results obtained may have been unfavourably influenced by a weakening of the vaccine, brought about by repeated re-sterilisation." In no case was reinoculation done. The statistics were compiled from information furnished by officers of the Royal Army Medical Corps actually in charge of troops in the various stations; and were supplemented by reports received from the commanding officers of the various inoculated regiments. They are as follows:—

Numbers under Observation.	Cases.	Deaths.	Percentage of Cases.	Percentage of Deaths.
Inoculated . 2835	27	5	0.95	0.2
Uninoculated 8460	213	23	2.5	0.34

If the inoculated had been attacked equally with the uninoculated throughout the period of observation, they would have had 71 cases instead of 27.

These inoculations belong to the early part of 1899. During the rest of the year, inoculations were made in India, Egypt, and Malta: the results are given in an appendix to the Report of the Royal Army Medical Department, 1899. (See *British Medical Journal*, 21st September 1901.) The great majority of the troops tabulated were in India. Of the troops stationed at

Malta, 61 were inoculated, 2456 not inoculated; among the former there were no cases, among the latter there were 17 cases and 5 deaths. In Egypt, of 4835 troops, 461 were inoculated; among these there were no cases, among the uninoculated there were 30 cases and 7 deaths. In India, of 30,353 troops, 4502 were inoculated, leaving 25,851 not inoculated; among the inoculated there were 44 cases and 9 deaths, among the non-inoculated 657 cases and 146 deaths. Taking the Indian statistics, and estimating percentage to strength, we find, amongst the inoculated, admissions 0.98, deaths 0.2; amongst the non-inoculated, admissions 2.5, deaths 0.56. The cases which occurred amongst the inoculated men were in the majority of instances of a mild character. Taking Malta, Egypt, and India together, it appears that the inoculated, if they had suffered equally with the non-inoculated, would have had 108 cases and 24 deaths, instead of 44 cases and 9 deaths.

At the end of 1899, this treatment, only just out of the hands of science, was suddenly demanded for the protection of a huge army at war in a country saturated with typhoid. Still, the South African results, and other results during 1899 to 1901, show a good balance of lives saved. The following paragraphs give all results published from the beginning of 1900 to May 1902. They are put in order of publication. Doubtless a few other reports have been overlooked in compilation; but the list includes all that were easily accessible.

1. *Manchester, England.* The *British Medical Journal*, 28th April 1900, contains a note by Dr. Marsden, Medical Superintendent of the Monsall Fever Hospital, Manchester, on the inoculation of 14 out of 22 nurses

engaged in nursing typhoid patients. Of the remaining 8, 4 had already had typhoid. The inoculations were made in October 1899. The following table shows the subsequent freedom from typhoid of the nursing staff:—

Year.	Number of Typhoid Patients.	Cases among Nursing Staff.
1895	229	3
1896	238	3
1897	302	4
1898	426	8
To end of September 1899	163	5
From October 1899 to March 1900	146	0

2. *Ladysmith, South Africa.* The *Lancet*, 14th July 1900, contains a short note by Professor Wright, on the distribution of typhoid among the officers and men of the military garrison, during the siege of Ladysmith. The figures are as follows:—

	Number.	No. of Cases.	Proportion of Cases.	No. of Deaths.	Proportion of Deaths.	Case-mortality.
Not inoculated	10,529	1489	1 in 7.07	329	1 in 32	1 in 4.52
Inoculated .	1,705	35	1 in 48.7	8	1 in 213	1 in 4.4

The wide difference between the two groups, as regards the incidence of the disease, is well marked; but the case-mortality is practically the same in each group. (The statistics of the General Hospital, Ladysmith, also tell in favour of the preventive treatment: see Surgeon-Major Westcott's letter, *British Medical Journal*, 20th July 1901, in answer to Dr. Melville's letter, *British Medical Journal*, 20th April 1901.)

3. *The Portland Hospital: Modder River and Bloem-*

fontein. The *British Medical Journal,* 10th November 1900, contains an account by Dr. Tooth of the cases of typhoid in this hospital. Concerning the preventive treatment, he says: "The experience of my colleague Dr. Calverley and myself may be of interest, though we fear that the numbers are too few for safe generalisation.

"*Personnel of the Portland Hospital.* We take first the relation of disease and inoculation among the *personnel* of the hospital. Twenty-four non-commissioned officers, orderlies, and servants of the Portland Hospital, and 4 of the medical staff, were inoculated on the voyage out. All these showed the local symptoms at the time; that is, pain, stiffness, and local erythema; 17 also presented well-marked constitutional symptoms—general feeling of illness, fever, and headache. Of the orderlies, 9 had enteric fever subsequently. Two had refused inoculation, and both of these had the disease very severely; in fact one died. Of the inoculated cases, 5 had the disease lightly, and 2 fairly severely. One of the sisters had the disease rather severely, and she had not been inoculated.

"*Officers and men admitted to the Portland Hospital.* We had under treatment at the Portland Hospital 231 cases of enteric fever, most of which came under our care at Bloemfontein. We have not included in these figures a number of patients who came in convalescent for a short time only, and on their way to the base, and who would therefore appear in the admission and discharge book of the hospital. If we did so, of course our percentages would be lower. Of these 231 patients, 53 had been inoculated at home or on the voyage out, and of them 3 died, making a percentage of deaths among the inoculated of 5.6 per cent.; 178 had not been inoculated, of whom 25 died; that is, a

mortality among the non-inoculated of 14 per cent. The general mortality in enteric fever with us was 28 deaths out of 231 cases; that is, 12.1 per cent., which seems to compare favourably with the experience of the London hospitals.

"It is interesting to record our experience among the officers taken separately. Thirty-three officers were admitted with enteric fever; 21 had been inoculated; that is, 63.6 per cent., a much larger percentage than among the men. Only one of these officers died, and he had not been inoculated.

"These figures are small, but such as they are they are significant, and they dispose us to look with favour upon inoculation. So also does our clinical experience with our patients, for among the inoculated the disease seemed to run a milder course."

4. *No. 9 General Hospital, Bloemfontein.* The *Medical Chronicle* for January 1901 contains an account, by Dr. J. W. Smith, of the work of this hospital. He says: "The general impression amongst the medical officers in our hospital was that a single inoculation probably did not confer an immunity lasting very long—the lapse of time differing in individuals—and also that there was a tendency in the cases of enteric in inoculated patients to abort at the end of ten or fourteen days. I should say, however, that a very considerable number of our detachment who had been inoculated suffered from enteric, of whom 4 at least died. Of the medical staff, the only member of the junior staff who had not been inoculated died of enteric."

5. *Scottish National Red Cross Hospital, Kroonstadt.* The *British Medical Journal*, 12th January 1901, contains an account of the work of this hospital by Surgeon-Colonel Cayley, Officer in Charge. He says:

"The first section of the hospital, consisting of 61 persons—officers, nursing sisters, and establishment—left Southampton on 21st April 1900. During the voyage out, all except 4 were inoculated twice, at an interval of about ten days; 2 were inoculated once; and 2 (who had had typhoid) were not inoculated. Immediately we reached the Cape, the hospital was sent up to Kroonstadt in the Orange River Colony, and remained there as a stationary hospital till the middle of October. During this period there were always many cases of enteric under treatment in hospital. Further, some of the medical officers and student orderlies had charge of the Kroonstadt Hotel temporary hospital, which was crowded up with enteric cases; and the nursing sisters, for three weeks, did duty in the military hospitals at Bloemfontein in May and June, when enteric fever was at its worst. There was not a single case of enteric among the *personnel* of this first section of the hospital.

"The second section of the hospital—medical officers, nurses, and establishment, 82 in all—left Southampton in May 1900. On board ship nearly all of them were inoculated, but many of them only once. The material for inoculation had been on board for some time, and was not so fresh as in the first instance. Of this second section, 1 nurse had enteric at Kroonstadt. She was the only one, out of a total of 36 nurses, who suffered from enteric; and she was the only nurse who was not inoculated, excepting the 2 who were protected by a previous attack of enteric. A third section of the hospital, consisting of 4 medical officers and 16 nurses, went out in July; they were all inoculated, and none of them had enteric.

"Of the second section, 5 orderlies had enteric fever

at Kroonstadt, of whom 2 died. Of these 5, there were 2 inoculated (once) and 3 non-inoculated. Of the 2 who died, 1 had been once inoculated, the other had not been inoculated."

6. *Meerut, India.* The *British Medical Journal*, 9th February 1901, gives a short note by Professor Wright on inoculations in the 15th Hussars. He says: "Through the kindness of Lieutenant-General Sir George Luck, commanding the Bengal Army, I am permitted to publish the following officially compiled statistics, dealing with the effects of anti-typhoid inoculations in the case of the 15th Hussars:—

From 22nd October 1899 to 22nd October 1900.

	Strength.	Inoculated.	Cases.	Deaths.	Not Inoculated.	Cases.	Deaths.
Officers. . .	22	19	0	0	3	0	0
N.C.O. and Men .	481	317	2	1	164	11	6
Women. . .	36	24	0	0	12	0	0

It would thus appear that the incidence of enteric in the inoculated was represented by 0.55 per cent., and the mortality by 0.27 per cent.; while the incidence in the uninoculated was 6.14 per cent., and the death-rate 3.35 per cent."

If the inoculated had suffered equally with the uninoculated, they would have had 22 cases with 11 deaths, instead of 2 cases with 1 death.

7. *The Edinburgh Hospital, South Africa.* The *Scottish Medical and Surgical Journal*, March 1901, contains an account of the work of the Edinburgh Hospital, by Dr. Francis Boyd. Of the staff, 58 were inoculated (27 once, and 31 twice). Among these 58, there were

Journal, 26th October 1901, contains a note by Professor Wright on an outbreak of typhoid in this asylum during August to December 1900. Inoculations were begun on 6th September, by Dr. Cullinan, and by 30th November 511 persons were inoculated. After careful criticism of all doubtful cases, Professor Wright gives the following figures:—

Comparative Incidence of Typhoid Fever in Inoculated and Non-Inoculated, calculated upon the average strength of the representative groups during the period intervening between the commencement of the inoculations and the termination of the epidemic.

	Average Strength.	Cases.	Deaths.	Percentage of Cases.	Percentage of Deaths.
Uninoculated .	298	30(−1?)	4	10.1	1.3
Inoculated .	339	5(+1?)	1	1.3	0.3

"It may be noted," he says, "that the result is in conformity with that of all the statistical returns of anti-typhoid inoculation which have reached me."

11. *Deelfontein.* The *Lancet*, 18th January 1902, contains a paper by Dr. Washbourn and Dr. Andrew Elliot, on 262 cases of typhoid fever in the Imperial Yeomanry Hospital at Deelfontein during the year March 1900 to March 1901. (See Dr. Washbourn's earlier letter, *Brit. Med. Jour.*, 16th June 1900.) They say: "In 211 of our cases, it was definitely recorded whether the patient had been inoculated or not: 186 of these cases had not been inoculated, with 20 deaths, or a mortality of 10.7 per cent.; 25 had been inoculated, with 4 deaths, or a mortality of 16 per cent. The mortality was thus higher among the inoculated than among the non-inoculated." Of the *personnel* of the hospital, there were 59 inoculated, with 4 cases, and 25 not inoculated, with 4 cases.

12. *Winburg.* The *Lancet*, 5th April 1902, contains a short note by Professor Wright, on the 5th Battalion, Manchester Regiment. He says: "In view of the dearth of statistics bearing on the incidence of typhoid fever in South Africa in inoculated and uninoculated persons respectively, the following, for which I am indebted to Lieutenant J. W. West, R.A.M.C., Winburg, Orange River Colony, may not be entirely without interest. The statistics here in question give the results obtained in the case of the 5th Battalion, Manchester Regiment, for the six months which have elapsed since their landing in South Africa. The figures, which relate to a total strength of 747 men and officers under observation, are as follows:—

	Number.	Cases.	Deaths.	Percentage of Cases.	Percentage of Deaths.
Uninoculated . .	547	23	7	4.2	1 in 3.3
Inoculated . . .	200	3	0	1.5	0

"The three attacks in the inoculated are reported to have been of exceptionally mild type, contrasting in a striking manner with the severe attacks which occurred in the uninoculated. At the time of sending in the report, some of the uninoculated patients were 'not yet out of danger.'"

Certainly, these instances show a good balance of lives saved, not only under the adverse conditions of the war, but also in Egypt, India, and the United Kingdom. But the bacteriological work on typhoid fever has been directed also to the working out of a

very different problem: and that is the method of diagnosis which is called "Widal's reaction." The practical uses of this reaction are of the utmost importance. It is the outcome of work in different parts of the world—by Wright and Semple and Durham in England, Chantemesse and Widal in France, Pfeiffer and Kolle and Grüber in Germany, and many more. The first systematic study of it was made by Durham and Pfeiffer; and Widal's name is especially associated with the application of their work to the uses of practice. Admirable accounts of the whole subject are given by Dr. Cabot in his book, *The Serum-Diagnosis of Disease* (Longmans, 1899), and by Mr. Foulerton in the *Middlesex Hospital Journal*, October 1899 and July 1901.

Widal's reaction is surely one of the fairy tales of science. The bacteriologist works not with anything so gross as a drop of blood, but with a drop of blood fifty or more times diluted; one drop of this dilution is enough for his purpose. Take, for instance, an obscure case suspected to be typhoid fever: a drop of blood taken from the finger is diluted fifty or more times, that the perfect delicacy of the test may be ensured; a drop of this dilution is mixed with a drop of nutrient fluid containing living typhoid bacilli, and a drop of this mixture of blood and bacilli is watched under the microscope:—

"The motility of the bacilli is instantaneously or very quickly arrested, and in a few minutes the bacilli begin to aggregate together into clumps, and by the end of the half-hour there will be very few isolated bacilli visible. In less marked cases, the motility of the bacilli does not cease for some minutes; while in the least marked ones the motility of the bacilli may never be

completely arrested, but they are always more or less sluggish, while clumping ought to be quite distinct by the end of the half-hour."

The result of this clumping is also plainly visible to the naked eye, by the subsidence of the agglutinated bacteria to the bottom of the containing vessel: and thus an easy practical mode of diagnosis is afforded by it.

As with typhoid, so with Malta fever, cholera, and some other infective diseases. And the unimaginable fineness of this reaction goes far beyond the time of the disease. Months, even years, after recovery from typhoid, a fiftieth part of a drop of the blood will still give Widal's reaction: and it has been obtained in an infant whose mother had typhoid before it was born. A drop of dried blood, from a case suspected to be typhoid, may be sent a hundred miles by post to be tested; and typhoid, like diphtheria, may now be submitted to the judgment of an expert far away, and the answer telegraphed back. It would be difficult to exaggerate the practical importance of this reaction for the early diagnosis of cases of typhoid fever, especially those cases that appear, at the onset, not severe.

Malta Fever

The specific organism of Malta fever (Mediterranean fever), the *bacillus Melitensis*, was discovered in 1887 by Surgeon-Major David Bruce, of the Army Medical Staff. Its nature and action were proved by the inoculation of monkeys. The use of Widal's reaction is of great value in this disease:—

"The diagnosis of Malta fever from typhoid is, of course, a highly important practical matter. It is exceedingly difficult in the early stages." (Manson, *loc. cit.*)

As with typhoid, so with Malta fever, Netley led the way to the discovery of an immunising serum. In the course of the work, one of the discoverers was by accident infected with the disease:—

"He was indisposed when he went to Maidstone to undertake anti-typhoid vaccination, and after fighting against his illness for some days, he was obliged to return to Netley on 9th October. Examination of blood-serum (Widal's reaction) showed that he was suffering from Malta fever. It appears that he had scratched his hand with a hypodermic needle on 17th September, when immunising a horse for the preparation of serum-protective against Malta fever; and his blood, when examined, had a typical reaction on the micrococcus of Malta fever in 1000-fold dilution. The horse, which has been immunised for Malta fever for the last eight months, was immediately bled, and we are informed that the patient has now had two injections, each of 30 cub. cm. of the serum. He is doing well, and it is hoped that the attack has been cut short." (*British Medical Journal*, 16th October 1897.)

About fifty cases had up to September 1899 been treated at Netley "with marked benefit: whereas they found that all drug-treatment failed, the antitoxin treatment had been generally successful."[1] A good instance of the value of the serum-treatment of Malta fever is published in the *Lancet*, 15th April 1899. For a later

[1] For the whole subject, see *Lancet*, 9th September 1899, paper by Surgeon-Major Birt and Surgeon-Captain Lamb. Two other cases of accidental inoculation occurred at Netley.

account of this treatment and of its efficacy, see the *Philadelphia Medical Journal*, 24th November 1900.

Another point is noted by Sir Patrick Manson, in his recent Lane Lectures (Constable, 1905). "For some time back," he says, "a commission of experts, working under the direction of the Royal Society, has been studying this disease in Malta. The commission has accumulated much detailed information; but the most important observation it has published is the fact that a large percentage of the goats in Malta are infected with *Micrococcus melitensis*, and that the milk of the infected goats contains the bacterium. May not this account for the great prevalence of Mediterranean fever there and in other places having perhaps a similar milk-supply?"

X

THE MOSQUITO: MALARIA, YELLOW FEVER, FILARIASIS

WITHIN the last few years, it has been proved that the mosquito is an intermediate host, between man and man, of malaria, yellow fever, and filariasis (elephantiasis).[1] Just as the grosser parasites, the tape-worms, must alternate between man and certain animals, and cannot otherwise go through their own life-changes and reproduce their kind, so the micro-parasites that are the cause of malaria alternate between man and the mosquito, having the mosquito as an intermediate host. These organisms, once they get into the mosquito, pick out certain structures, and there carry out a definite cyclical phase of their lives, whereby their progeny make their way into the stylets of the mosquito, and so get back to man, who is their "definite host." Thus, malaria is not, strictly speaking, a disease of man; it is one phase in man of micro-organisms that have another phase in mosquitoes. So also with filariasis; the filariæ in man, their ova, and their embryo-worms, are one phase of filariasis; and the embryo-worms in certain structures of the mosquito are another phase. The

[1] For Dr. Graham's experiments at Beyrout, which seem to prove that the mosquito can also convey dengue or dandy-fever, see the *New York Medical Record*, 8th February 1902.

plasmodium malariæ and the *filaria* are instances of a law of animal life that holds good also of plant life:—

"All plants and animals possess parasites, and thousands of different species of parasites have been closely studied by science; we therefore know much about their general ways of life. As a rule, a particular species of parasite can live only in the particular species of animal in which, by the evolution of ages, it has acquired the power of living. It is therefore not enough for the parasites of an individual animal—say a man—to be able to multiply within that individual, but they must also make arrangements, so to speak, for their progeny to enter into and infect other individuals of the same species. They cannot live for ever in one individual; they must spread in some way or other to other individuals.

"The shifts made by parasites to meet this requirement of their nature are many and various, and constitute one of the wonders of nature. Some scatter their spores and eggs broadcast in the soil, water, or air, as it were in the hope that some of them will alight by accident on a plant or animal suitable for their future growth. Many parasites employ, in various ways, a second species of animal as a go-between. Thus, some tape-worms, and the worms which cause trichinosis, spend a part of their lives in the flesh of swine, and transfer themselves to human beings when the latter eat this flesh. To complete the cycle, the parasites return to swine from human offal; so that they propagate alternately from men to swine, and from swine to men. The blood-parasites which cause the deadly tsetse-fly disease among cattle in South Africa are transferred from one ox to another on the proboscis of the ox-biting or tsetse-fly. The progeny of the flukes of sheep enter a kind of snail, which spreads the parasites upon grass. The progeny of the guinea-worm of man enter a water-flea. The progeny of the parasites which cause Texas cattle-fever, and which are very like the malarial parasites, live in cattle-ticks, and

are transferred by the young of these ticks into healthy cattle." (Ross, *Malarial Fever*, 1902.)

1. Malaria

The *plasmodium malariæ* was discovered by Laveran in 1880, in the blood of malarial patients. For many years his work stopped there, because it was impossible to find the *plasmodium* in animals: "the difficulties surrounding the subject were so great that this discovery seemed to be almost hopeless." In 1894, Sir Patrick Manson—who had proved mosquitoes to be the intermediate host in the case of the parasitic nematode *filaria*—suggested, as a working theory of malaria, that the plasmodium was carried by mosquitoes. This belief, not itself new, he made current coin. He observed that there is a flagellate form of the plasmodium, which only comes into existence after the blood has left the body: and he suggested that the flagella might develop in the mosquito as an intermediate host, a halfway-house between man and man. Then, in 1895, Ross set to work in India, keeping and feeding vast numbers of mosquitoes on malarial blood; and for two years without any conclusive result. About this time came MacCallum's observations, at the Johns Hopkins University, on a parasitic organism, *halteridium*, closely allied to the plasmodium malariæ; he showed that the flagella of the halteridium are organs of impregnation, having observed that the non-flagellated form, which he regarded as the female, after receiving one of the flagella, changed shape, and became motile. In August 1897, Ross found bodies, containing pigment like that of the malarial parasite, in the outer coat of the stomach of one kind of mosquito, the grey or dapple-winged

mosquito, *Anopheles maculipennis*, that had been fed on malarial blood. In February 1898, he was put on special duty under the Sanitary Commissioner with the Government of India, to study malaria, and started work again in Calcutta :—

"Arriving there at a non-fever season, he took up the study of what may be called 'bird malaria.' In birds, two parasites have become well known—(1) the halteridium, (2) the proteosoma of Labbé. Both have flagellated forms, and both are closely allied to the plasmodium malariæ. Using grey mosquitoes and proteosoma-infected birds, Ross showed by a large number of observations that it was only from blood containing the proteosoma that pigmented cells in the grey mosquito could be got; therefore that this cell is derived from the proteosoma, and is an evolutionary stage of that parasite. Next, Ross proceeded to find out its exact location, and found that it lay among the muscular fibres of the wall of the mosquito's stomach. It grows large (40–70 micromillimetres) and protrudes from the external surface of the stomach, which under the microscope appears as if covered with minute warts." (Manson, at Edinburgh meeting of British Medical Association, 1898.)

These pigmented spherical cells give issue to innumerable swarms of spindle-shaped bodies, "germinal rods"; and in infected mosquitoes Ross found these rods, in the glands that communicate with the proboscis. Thus the evidence was complete, that the plasmodium malariæ, like many other parasites, has a special intermediate host for its intermediate stage of development; and that this host is the dapple-winged mosquito. It is impossible to over-estimate the infinite delicacy and difficulty of Ross's work; for instance, in his "Abstract of Recent Experiments with Grey Mosquitoes," he says that "out of 245 grey mosquitoes fed on birds

with proteosoma, 178, or 72 per cent., contained pigmented cells; out of 249 fed on blood containing halteridium, immature proteosoma, &c., not one contained a single pigmented cell." Another time (April 1898) he counted these pigment-cells under the microscope:—

"Ten mosquitoes fed on the sparrow with numerous proteosoma contained 1009 pigmented cells, or an average of 101 each. Ten mosquitoes fed on the sparrow with moderate proteosoma contained 292 pigmented cells, or an average of 29 each. The mosquitoes fed on the sparrow with no proteosoma contained no pigmented cells."

Finally, he completed the circle of development by infecting healthy sparrows by causing mosquitoes to bite them.

In 1899, there went out a German Commission to German East Africa, a Royal Society's Commission to British Central Africa, and an expedition from the Liverpool School of Tropical Medicine; in 1900, another German Commission, this time to the East Indies, and another expedition from the Liverpool School; by July 1901, the Liverpool School was organising its seventh expedition. Italy, of course, has given infinite study to the disease:—

"It has been decided that, in addition to the stations of observation and experiment in the provinces of Rome, Milan, Cremona, Mantua, Gercara, Foggia, Lecce, others shall be established in the provinces of Udine, Verona, Vicenza, Padua, Ravenna, Pisa, Basilicata, and Syracuse. Besides epidemiological researches, applications on a large scale will be made of preventive measures for the protection of the agricultural population against the scourge. Another extensive experiment on the prophy-

laxis of malaria will be made on the Emilian littoral. Moreover, in all the malarious regions of the Italian peninsula the provincial and communal administrations and many private persons will co-operate in the application of preventive measures. From all this it may be gathered that during the summer and autumn the war against malaria will be carried on in Italy with great vigour and thoroughness." (*British Medical Journal*, 6th July 1901.)

In India, the work started in 1900 by the Royal Society Commissioners, and by the Nagpur Conference, has been widely extended; especially by such researches as those of Major Buchanan, I.M.S., Superintendent of the Central Jail, Nagpur. The following paragraph, from the report of the Sanitary Commissioner with the Government of India, refers to Major Buchanan's published work, *Malarial Fevers and Malarial Parasites in India*:—

"A remarkable note is struck at the outset, in the acknowledgment made, by the author, of the capable assistance rendered in these researches by several of his Burmese prisoners, whom he trained to the use of the microscope, and who soon became expert in detecting and distinguishing the various kinds of parasites. . . . Besides a systematic clinical account of the different forms of fever and the associated parasites, which is the first attempt of the kind in India, there are a summary of the facts showing the relation of the seasonal prevalence of *Anopheles* to the incidence of attacks; experiments exhibiting the protective effects of mosquito-curtains; inoculation-experiments; researches on the blood-parasites of birds; and many other points. . . . Nor can we pause to notice the many attempts now being made by health officers and others to pursue the methods of prophylaxis indicated; these efforts are necessarily in the tentative stage, but, so far,

and especially where carried out in connection with small communities and institutions, they are giving promise of gratifying success."

The famous experiment made by Dr. Sambon and Dr. Low in 1900, must be recalled here:—

"Dr. Luigi Sambon and Dr. G. C. Low, both connected with the London School of Tropical Medicine, volunteered to live from June till October, that is to say, through what may be called the height of the malaria season, in a part of the Campagna near Ostia, which is so infested by the disease that no one who spends a night there under ordinary conditions escapes the effect of the poison. Dr. Sambon, Dr. Low, Signor Terzi, and their servants, have now exposed themselves to the pestilential influence of this valley of the shadow of death for over two months. They live in a mosquito-proof hut; they take no quinine or other drug which might be regarded as prophylactic. Not one of the experimenting party has the least sign of infection.[1] . . .

[1] Sir Patrick Manson, in the *British Medical Journal*, 29th September 1900, gives the following account of this experiment:—"A wooden hut, constructed in England, was shipped to Italy and erected in the Roman Campagna, at a spot ascertained by Dr. L. Sambon, after careful inquiry, to be intensely malarial, where the permanent inhabitants all suffer from malarial cachexia, and where the field-labourers, who come from healthy parts of Italy to reap the harvest, after a short time all contract fever. This fever-haunted spot is in the King of Italy's hunting-ground near Ostia, at the mouth of the Tiber. It is waterlogged and jungly, and teems with insect life. The only protection employed against mosquito-bite and fever by the experimenters who occupied this hut was mosquito-netting, wire screens in doors and windows, and, by way of extra precaution, mosquito-nets round their beds. Not a grain of quinine was taken. They go about the country quite freely—always, of course, with an eye on *Anopheles*—during the day, but are careful to be indoors from sunset to sunrise. Up to 21st September, the date of Dr. Sambon's last letter to me, the experimenters and their servants had enjoyed perfect health, in marked contrast to their neighbours, who were all of them either ill with fever, or had suffered malarial attacks."

"What for practical purposes may be regarded as an experiment of the same kind is being conducted in West Africa. Dr. Elliot, a member of the Liverpool expedition sent to Nigeria some time ago to investigate the subject of malarial fever, has recently returned to this country. He reports that the members of the expedition have been perfectly well, although they have spent four months in some of the most malarious spots. They lived practically amongst marshes and other places hitherto supposed to be the most deadly. They have not kept the fever off by the use of quinine, and they attribute their immunity to the careful use of mosquito-nets at night." (*British Medical Journal*, 22nd September 1900.)

A similar "experiment," of the utmost importance, was made in 1900 by Professor Grassi. It concerned the workmen and their families along the Battipaglia-Reggio railway, 104 in all, including 33 children. The great majority of them had suffered from malaria in the preceding year; and only 11, including 4 children, had never suffered from it. Pending the arrival of the malarial season, quinine was given to all who needed it. The first *Anopheles* with its salivary glands infected was found on 14th June. Twelve days later came a case of malaria outside the "zone of experiment," in a person who had never had malaria before. The twelve days correspond to the incubation-period after infection. *Anopheles* having come, and the malarial season with him, the experiment was begun. The houses were carefully protected with wire netting, chimneys and all; the *siesta* was taken under wire netting; the workmen, if they were out in the evening or at night, wore veils and gloves; and *Anopheles* was to be killed wherever he was found. Quinine was altogether given up and forbidden, except for three workmen who had escaped or evaded its use

before June, and had, indeed, never before been treated with quinine; one of them, moreover, had been sleeping outside the zone of experiment in July. Except these three, all the 104 and their doctors remained absolutely free from malaria up to 16th September, the date of Professor Grassi's report:—

"Rightly to estimate the value of these facts, it is necessary briefly to describe the surroundings of the protected area. Towards the north, coming from Battipaglia, three railway cottages are situated, at a distance of 1, 2, and 3 kilometres respectively. The 25 inhabitants of these cottages, although they were put under the tonic and quinine treatment in the non-malarial season, all without exception were taken ill with malarial fevers, in many cases obstinate."

Experiments of voluntary exposure to bite from an infected mosquito were made at or about this time, in London, New York, Italy, and India. The London "consignment" of mosquitoes had been allowed to bite a malaria-patient in Rome. The experiment had to be very carefully planned:—

"To have sent mosquitoes infected with malignant tertian parasites might have endangered the life of the subject of the experiment; and quartan-infected insects might have conferred a type of disease which, though not endangering life, is extremely difficult to eradicate. The cases, therefore, on which the experimental insects were fed had to be examples of pure benign tertian—a type of case not readily met with in Rome during the height of the malarial season; the absolute purity of the infection could be ascertained only by repeated and careful microscopic examination of the blood of the patient." (*British Medical Journal*, 29th September 1900.)

The mosquitoes were forwarded, through the British Embassy in Rome, to the London School of Tropical Medicine. The two brave gentlemen who let themselves be bitten by some thirty of the mosquitoes were in due time attacked by malaria, and the tertian forms of the parasite were found in their blood. Nine months later, one of them had a relapse, and the parasite was again found in his blood.

It is not possible to sum up the wealth of work on malaria published in 1900–1901. Good accounts of it are in the Transactions of the Section of Tropical Diseases, at the Annual Meeting of the British Medical Association (Cheltenham, 1901), and in the Thompson Yates Laboratories Reports, vol. iii., pt. 2, 1901. Everything had to be studied: not only the nature and action of the *plasmodium* in all its phases, but also the whole natural history and habits of the *Anopheles* of different countries; and, above all, the incidence of the disease on natives and on Europeans in China, India, and Africa. All that can be done here is to try to indicate the principal lines followed in the present world-wide campaign against malaria. The following paragraphs are taken mostly from the accounts given by Dr. Christophers and Dr. Annett, in the Thompson Yates Laboratories Report, 1901:—

1. *Elimination of the Infection at its Source.* This is the method employed with success by Professor Koch in New Guinea, viz., to search out all cases of malaria (the concealed ones in particular), and to render them harmless by curing them with quinine. At Stephansort, by thus hunting up all infected cases, and as it were, sterilising them by the systematic administration of quinine, he was able to achieve a great reduction of the disease in the next malarial season, even under

adverse conditions. He says, in his report to the German Government: "The results of our experiment, which has lasted nearly six months, have been so uniform and unequivocal that they cannot be regarded as accidental. We may assume that it is directly owing to the measures we have adopted that malaria here has, in a comparatively short time, almost disappeared."

This method, of course, is applicable only in small communities; and, within these limits, it may become one of the most valuable of all methods, being, like the quality of mercy, a blessing both to him who gives and to him who taketh. But it cannot be practised on a vast scale. This difficulty is well put by Sir William MacGregor, K.C.M.G., Governor of Lagos, West Africa:—

"In all probability, the day will come before long, when newly-appointed officers for places like Lagos will have to undergo a test as to whether they can tolerate quinine or not. A man that cannot, or a man that will not, take quinine, should not be sent to or remain in a malarial country, as he will be doing so at the risk of his own life, *and to the danger of others*. . . . The great difficulty is how to extend this treatment beyond the service, more particularly to the uneducated masses of the natives. It is simply impossible to protect the whole population by quinine administered as a prophylactic. In the first place, the great mass of natives would not take the medicine; and, in the second place, the Government could not afford to pay for the 70 tons of quinine a year that would be required to give even a daily grain dose to each of 3,000,000 of people."

2. *Segregation of Europeans from Natives.* This method is strongly advocated by the members of the Nigeria Expedition of the Liverpool School (1900). The distance of removal to half a mile is considered

sufficient: "Considerable evidence has now been accumulated to prove that the distance which is traversed by a mosquito is never very great, and extremely rarely reaches so much as half a mile." The arguments in favour of this method of "segregation" are of so great interest that they must be put here at some length. The drawback is that the method cannot be followed everywhere to its logical issue without some risk of giving offence, of seeming to abandon the native, of damaging commerce, and so forth. But, short of this, much might be done for the protection of Europeans in Africa:—

"This method is a corollary of the discovery that native children in Africa practically all contain the malaria parasite, and are the source from which Europeans derive malaria. Koch showed in New Guinea that in most places infection was very prevalent in native children, so much so that in some villages 100 per cent. of those examined contained parasites. He also showed that, as the children increased in age, immunity was produced, so that in the case of adults a marked immunity was present, and malarial infection was absent. The Malaria Commission showed, independently, that a condition of universal infection existed among the children of tropical Africa, associated with an immunity of the adults. This infection in children had many remarkable characteristics. The children were in apparent health, but often contained large numbers of parasites, and a small proportion only of the children failed to show some degree of infection. . . . The Liverpool School Expedition found a similar condition of affairs in all parts of Nigeria visited by them.

"With a knowledge of the ubiquity of native malaria, the method of infection of Europeans becomes abundantly clear. The reputed unhealthiness or healthiness of stations is seen at once to be dependent on the proximity

or non-proximity of native huts. The attack of malaria after a tour up-country, the malaria at military stations like Prah-su, the abundance of malaria on railways, are all explicable when the extraordinary condition of universal native infection is appreciated. It is evident that, could Europeans avoid the close proximity of native huts, they would do away with a very obvious and great source of infection. . . . When it is understood that each of these huts certainly contains many children with parasites in their blood, and also scores or hundreds of *Anopheles* to carry the infection, then the frequency with which Europeans suffer from malaria is scarcely to be wondered at. . . . The accompanying plan is that of a new railway settlement on the Sierra Leone Railway. Miles of land free from huts exist along the line, but the close neighbourhood of native huts has been selected. At the time of building of these quarters, it lay in the power of the engineers to have a malaria-free settlement; instead of which, by the non-observance of a simple fact, the station is most malarious: in this particular instance, much ingenuity has been shown in providing each set of European quarters with plenty of malarial infection. In towns only is there any difficulty in carrying out the principle of segregation. In two instances, however, this has been carried out in towns, with the result that the segregated communities of Europeans are notoriously the most healthy on the West Coast. Even when no scheme of complete segregation can be carried out, the principle should always be borne in mind, and, whenever opportunity offers, huts should be removed, and European houses built in the open. . . . It is almost universally the rule in West Africa to find European houses built round by native quarters, a practice which long experience in India has taught Europeans to avoid carefully. At Old Calabar, many of the factories are almost surrounded, except in front, by native habitations; similarly, at Egwanga, the small native town is built by the side and back of one of the factories. Also at the Niger Company's factory at Lokoja, the native houses are very close up

to the Company's boundary railings. Akassa engineers' quarters may be, again, mentioned as an example where the engineering artisans, chiefly natives of Lagos, Accra, and Sierra Leone, are housed with their families alongside the European house. A large proportion of these native children were found by us to contain malarial parasites. Similarly also at Asaba, the proximity of the barracks of the Hausa soldiers, who have their wives and children with them, is a dangerous menace to the officers at the Force House.

"Examples of the opposite condition of affairs might also be given. For instance, at Old Calabar, the Government offices and Consulate, Vice-Consulate, and medical house, are comparatively free from malarial fever; it having been established that the natives shall not build on the European side of the creek separating the two slopes on which the native town and European quarters are built. This creek is at a distance of about half a mile from the houses mentioned."

It is plain, from these and other instances given by the members of the Nigeria Expedition, that a modified sort of "segregation" can be effected in many places, without any injury either to native feelings, or to politics, or to commerce; and that by such segregation the risk of malaria among Europeans in Africa would be diminished.

3. *Protection against Anopheles.* Manson, in his *Tropical Diseases* (1905), says, "The question is often asked, Is there any other way by which malaria can be contracted than through a mosquito-bite? For many reasons, I believe not. It is difficult to prove a negative; but, so far, there is no observation capable of bearing investigation that would lead us to suppose that malaria can be acquired, under natural conditions, except by mosquito-bite." All authorities are agreed

that, practically, the fight against malaria and the fight against *Anopheles* are one and the same thing; and the experiments by Sambon, Low, and Grassi, show what can be done, in this war against the mosquito, by way of defence. But what is practicable in Italy might not be generally practicable on the West African coast; as Sir William MacGregor says of Lagos:—

"It is not likely that in a place like Lagos as good results can be obtained from the use of mosquito-proof netting as in Italy. One great objection to it here is the serious and highly disagreeable way it checks ventilation. This is a difficulty that cannot be fully brought home to one in a cold climate. But, in a low-lying, hot, and moist locality like Lagos, it comes to be a choice of evils, to sit inside the netting stewed and suffocated, or to be worried and poisoned by mosquitoes outside. The netting is hardly a feasible remedy as regards native houses. It is not possible to protect even European quarters completely by it. Few officers or others are so occupied that they could spend the day in a mosquito-proof room. Certain it is that any man that suffers from the singular delusion that mosquitoes bite only during the night, would have a speedy cure by spending a few days, or even a few hours, in Lagos. Operations here (September 1901) are being limited to supplying one mosquito-proof room to the quarters of each officer. In this he will be able to spend the evening free from mosquitoes if he chooses to do so. The European wards of the hospital are similarly protected."

The European in Africa, as Ross says, is generally neglectful of his health; and the "unhealthiness" of the African coast is to some extent due to the life that men lead there:—

"Let us compare the habits of a European in a business-house in Calcutta with the habits of a European in

West Africa. In Calcutta he sleeps under a punkah or mosquito-net, or both; he dresses and breakfasts under a punkah; in the evening he takes vigorous exercise, and he dines under a punkah. He wears the lightest possible clothing, he lives in a solid, cool, airy house, and he obtains very good food; once in five or six years, he returns to Europe for leave. . . . In Africa, the houses are frequently very bad; in Freetown, for instance, they are the same as the houses of natives, and are mingled with them. The Anglo-African seems to imagine that he can live in the tropics in the same manner as he lives in England. He seldom uses a punkah, except perhaps for an hour at dinner-time, and, not seldom, he neglects even the mosquito-net. The food is often, or generally, execrable. Owing to the frequent absence of gymkhanas and clubs, the exile obtains little suitable exercise."

But whatever risks the old resident may choose to take, the newcomer can at least use a proper and efficient mosquito-net at night, and avoid sleeping in a native house, and protect himself in these and the like ways against malaria.

4. *The keeping down of Anopheles.* The breeding places of *Anopheles* are ponds, swamps, and puddles, roadside ditches, tanks, and cisterns, old disused canoes, and the like collections of stagnant water: also the smaller receptacles that are more generally occupied by *Culex*, such as broken bottles, old tins, pots, and calabashes, and barrels, whatever will hold water—all the débris and broken rubbish round huts or houses. In all these places, *Anopheles*' eggs or larvæ are found; and, with practice, it is easy to detect them. Of course, it is not easy to wage war against the adult mosquito: the work is, *Venienti occurrere morbo*, to organise gangs of workmen, or of prison labour, and

"mosquito brigades"; to clear the ground of cartloads of old biscuit-tins, broken gin-bottles, and other dust-heap things, in and around the place; to cover-in the cisterns, rain-barrels, and wells; to clean pools and duck-ponds of weed, and stock them with minnows; to put a film of kerosene to the puddles, or sweep them out, or fill them up and turf them over; everywhere, to drain, and level, and clean-up the surface soil; and everywhere, by these and the like methods, to break the cycle of the life of the *plasmodium malariæ*:—

"Draining and cultivation where the land will repay the expenditure, permanent and complete flooding where it will not, and such flooding is possible; proper paving of unhealthy towns, and the filling-in of stagnant, swampy pools; these—in other words, all measures calculated to keep down mosquitoes—are the more important things to be striven for in attempting the sanitation of malarious districts. In England, in Holland, in France, in Algeria, in America, and in many other places, enormous tracts of country, which formerly were useless and pestilential, have been rendered healthy and productive by such means." (Manson.)

And, short of such great enterprises as Government works of drainage, much has already been done, in many African towns, and in India, by the work of a few men and women: not only by practical sanitary improvements, but by insistent teaching and lecturing. For the admirable results recently obtained in Ismailia, Algeria, Formosa, and the Malay States, see the *Medical Annual*, 1905 and 1906.[1]

[1] This paper, by Dr. Stephens, gives also the reasons why equally good results were not obtained at Mian Mir, Punjab. The whole paper is of great interest.

Before leaving the subject of malaria, it must be added that the discovery and study of the parasite which causes it have cleared up the mystery of the specific action of quinine upon the disease. It operates simply by its germicidal effect upon the microbe. But, beyond this, we have now a clue which we never had before to guide us to the most advantageous manner of administering the drug.

2. Yellow Fever

The specific organism of malaria may become active again and again in the blood, causing relapses twenty years or more after the original infection. The specific organism of yellow fever expends itself at once, in one acute attack; and, if the patient recovers, he is thenceforth more or less immune against infection. That the inoculation of the disease, by the application of a single mosquito recently contaminated, is calculated to produce a mild or abortive attack less dangerous than the average attack among the non-acclimatised, was known to Finlay, and was confirmed in 1899 by the Army Commission of the United States.

Of the mortality of the disease, Sir Patrick Manson, in 1900, wrote as follows:—

"It is better for women and children than for men; better for old residents than for newcomers; worst of all for the intemperate. According to a table of 293 carefully observed cases given by Sternberg, the mean mortality in the whole 293 cases was 27.7 per cent. This may be taken as a fairly representative mortality in yellow fever among the unacclimatised, something between 25 and 30 per cent., although in some epidemics it has risen as high as 50 or even 80 per cent. of those attacked. . . . Some

of these epidemic visitations bring a heavy death-bill; thus, in New Orleans, in 1853, 7970 people died of yellow fever; in 1867, 3093; in Rio, in 1850, it claimed 4160 victims; in 1852, 1943; and in 1886, 1397. In Havana, the annual mortality from this cause ranges from 500 to 1600 or over."

The earlier attempts to reproduce the disease, by inoculation with its products, failed altogether:—

"In 1816, Dr. Chervin, of Point-à-Pitre (Antilles), drank repeatedly large quantities of black vomit without feeling the least disturbance. Some years before, other North American colleagues, Doctors Potter, Firth, Catteral, and Parker, did everything possible to inoculate themselves with yellow fever. After having uselessly attempted experiments on animals, they experimented on themselves, inoculating the black matter at the very moment in which the moribund patient rejected it, placing this matter in their eyes, or in wounds made in their arms, injecting it more than twenty times in various parts of their body . . . in short, devising every sort of daring means for experimentally transmitting yellow fever. All these experiments were without result, and in the United States during many years it was believed that this terrible malady was non-contagious." (*British Medical Journal*, 3rd July, 1897.)

The history of the subject, from 1812 to 1880, is given by Dr. Finlay of Havana, in the *New York Medical Record* (9th February 1901). In 1880, two very important reports on the disease were published; one by a Havana Commission of the National Board of Health of the United States, the other by the United States Navy Department. They tended to show that yellow fever is a "germ-disease"; that it is not wind-borne; and that there may be some change, outside the body of the patient, whereby the virulence of the

active principle of the disease is heightened. From these reports, Dr. Finlay advanced his doctrine that the mosquito receives and transmits the germs of the disease :—

"It was upon the above line of reasoning (in these reports), that I conceived the idea that the yellow-fever germ must be conveyed from the patient to the non-immunes by inoculation, a process which could be performed in nature only through the agency of some stinging insect whose biological conditions must be identical with those which were known to favour the transmissibility of the disease."

In 1881 he inoculated himself and six soldiers with infected mosquitoes, and obtained, as he had calculated, mild attacks and subsequent immunity. During the years 1881–1900 he inoculated by this method 104 persons :—

"In these inoculations, be it remembered, my principal object was rather to avoid than to seek the development of a severe attack ; in point of fact, only seventeen showed any appreciable pathogenic effects after their inoculation. I felt sure, however, that severe or fatal result might follow an inoculation either with several mosquitoes contaminated from severe cases of the disease, or from a single insect applied several days or weeks after its contamination, having come to this last conclusion in view of the facts connected with the *Anne Marie*, and the epidemic of Saint Nazaire."

Dr. Finlay's discovery that the mosquito can convey yellow fever, and that the germ of the disease is more virulent after a prolonged sojourning in the mosquito, was proved beyond all question by the work of 1889–1901. But, so far as immunisation is concerned, few

development in the body of the mosquito before the latter is capable of conveying infection. This period would seem to be not less than twelve days.

"We also consider the question of house infection, and are able to show that this infection is due to the presence of mosquitoes that have previously bitten yellow-fever patients; and that the danger of contracting the disease may be avoided in the case of non-immune individuals who sleep in this building, by the use of a wire screen.

"We also demonstrate, by observations made at this camp (Fort Lazear), that clothes and bedding contaminated by contact with yellow-fever cases, or by the excreta of these cases, is absolutely without effect in conveying the disease."

In February 1901, Dr. H. E. Durham published an abstract of an *interim* report of the Liverpool School Yellow Fever Commission. He and Dr. Walter Myers, the two Commissioners, had both of them been attacked by the disease, and Dr. Myers had died of it. The report gives evidence that the disease is due to a bacillus which is not the *bacillus icteroides;* and it does not wholly favour the earlier report (1900) of the American Commission. A later Commission to New Orleans, September 1901 to January 1902, reported an extensive series of investigations, which seem rather to support the belief that the *bacillus icteroides* is the cause of the disease. Later still, this belief is again denied; and, as in rabies, so in yellow fever, the good work has gone on without waiting for the identity of this or that micro-organism.

Immunisation, by the direct use of an infected mosquito, may be compared with the old custom of

inoculation against smallpox. The use of Sanarelli's serum-treatment has not gone far. There remains for consideration the method of keeping down infection by keeping down *Culex*.

Three reports, in 1901–1902, come from Dr. Guitéras (Havana), Surgeon-Major Gorgas, chief sanitary officer (Havana), and the Commission at New Orleans. Dr. Guitéras reports that 6 cases of yellow fever (inoculation) were treated in a large "mosquito-proof" building, which also contained cases of other diseases. No prophylaxis was enforced, save the exclusion of mosquitoes; non-immunes visited the yellow fever cases, non-immunes nursed them, and most of the attendants and labourers about the place were non-immunes; but not a single case of infection occurred. The New Orleans Commission reports that, of 200 cisterns, &c., examined in the city for the presence of larvæ, the larva of *Culex* (*Stegomyia*) predominated in more than 60 per cent.

The report of Surgeon-Major Gorgas is very pleasant reading. For two centuries, Cuba had been cursed with yellow fever; then, after the war with Spain, America took it over:—

"The army took charge of the health department of Havana, when deaths (from all causes) were occurring at the rate of 21,252 per year. It gives it up, with deaths occurring at the rate of 5720 per year. It took charge, with smallpox endemic for years. It gives it up, with not a case having occurred in the city for over eighteen months. It took charge, with yellow fever endemic for two centuries — the relentless foe of every foreigner who came within Havana's borders, which he could not escape, and from whose attack he well knew every fourth man must die. The army has stamped out this disease in its greatest stronghold."

Make fair allowance for the wide variation, from year to year, of the number of yellow fever cases in any town within the geographical belt of the disease ; admit that a town may, in the course of nature, have many hundred cases in one year, and only half a dozen in another year. Again, make fair allowance for all other good influences of the American occupation of Cuba, beside those that were concerned with the stamping out of *Culex;* admit that the general death-rate of Havana, in the last February of Spanish rule (1898), was 82.32 per thousand, and in February 1901, was 19.32. Still, there is an example here, in the 1901 work in Havana, for the world to follow, wherever yellow fever exists. The following abstract of Surgeon-Major Gorgas' results was published in the *Practitioner,* May 1902, by Professor Hewlett, one of the foremost of English bacteriologists :—

"Commencing in February 1901, orders were issued that every suspected case of yellow fever should be screened with wire gauze at the public expense, so as to render the room or rooms mosquito-proof. All mosquitoes in the infected house and in contiguous houses were destroyed. After the middle of February, 100 men were employed in carrying out the destruction of the mosquito-larvæ in their breeding places, putting oil in the cesspools of all houses, clearing the streams, draining pools, and oiling the larger bodies of water. Up to June, quarantine was enforced, together with disinfection of the house and fomites. After that, however, rigid quarantine of the patient was stopped, and disinfection of fabrics and clothing ceased. It was merely required that the patient should be reported, his house placarded and screened, and a guard placed over each case to report how general sick-room sanitation was carried out, to see that the screen-door communicating with the screened part of the house was kept properly closed, and to see that communication

with the sick-room was not too free, four or five non-immunes only being allowed in. *By the end of September, the last focus of the disease had been got rid of, and since then, up to the beginning of January, there has not been a single case.* Whereas, for the years since 1889, from 1st April to 1st December, yellow fever caused an average of 410.54 deaths, with a maximum of 1175 for 1896, and a minimum of 79 for 1899, *it caused in* 1901 5 *deaths only. In the months of October and November, when the disease has hitherto been exceedingly rife in Havana, there has not been a single case. For the first time in* 150 *years, Havana has been free from yellow fever.*"

Sir Patrick Manson, lecturing in America, last year, on tropical diseases, summed up the work as follows:—

"Time will not permit—what to you is probably quite unnecessary — the recapitulation of the story of the labours of Reed and his coadjutors. I cannot pass on, however, to what I have to say in connection with this work without a word of admiration for the insight, the energy, the skill, the courage, and withal the modesty and simplicity of the leader of that remarkable band of workers. If any man deserved a monument to his memory, it was Reed. If any band of men deserve recognition at the hands of their countrymen, it is Reed's colleagues.

"The principal outcome of the labours of these men has been the demonstration, first, that the ultra-microscopic germ of yellow fever is present in the blood of the patient during the first three days of the disease. Second, that the first step in the passage of the germ from the sick to the sound is made, under natural conditions, in the stegomyia mosquito. And third, that after about twelve days and upwards in stegomyia, the yellow fever germ, when implanted by the said mosquito into another human host, is capable of reproduction, so that at the end of a further period of about three days it has established itself throughout the blood, is causing the violent re-

action, the clinical manifestations of which we call yellow fever, and is once more in a condition to re-enter the mosquito.

"These are great etiological facts. They are of supreme practical and scientific value. Acting on them, the United States sanitary authorities expelled yellow fever from Havana. Acting on them, they should be able in the future to protect the United States themselves from such terrible visitations as in the past have swept through some of your cities."

3. Filariasis

These same lectures contain an admirable account of the life-history of *Filaria*. It is not necessary here to describe the loathsome deformities which occur in the later stages of filariasis. These deformities (*elephantiasis*, Barbadoes leg), which may attain colossal size, are due to the blocking of the lymphatic vessels with filarial worms. Cases of the disease are hardly ever seen in this country; but it is very frequent in some parts of the tropics. *In the endemic areas*, says Manson, 10 *per cent. is not an uncommon proportion of the population to be found affected with filariasis. Thirty and even* 50 *per cent. may be affected. In many of the Pacific Islands—the Samoa group for instance—I believe that even this proportion is exceeded.*

That *Culex* (*fatigans*) can carry the parasite, has been proved past all doubt. Neither does anybody doubt, that the keeping down of this mosquito would keep down filariasis. A report of great interest, from Barbadoes, was published in the *British Medical Journal* for 14th June 1902. It is written by Dr. Low, whose experiment on himself in the Campagna has already been noted in this chapter. Dr. Low reports that there is no indigenous malaria in the island, and that neither

he nor Mr. Lefroy could find a single *Anopheles* larva, though they hunted diligently in the swamps and other likely places. But filariasis is terribly common, and so is *Culex fatigans*. Dr. Low examined the night-blood of 600 cases of all kinds in the General Hospital, the Central Almshouse, and elsewhere, and found the filaria-embryos in no less than $76 = 12.66$ per cent. He caught and dissected a hundred mosquitoes (*Culex fatigans*) from the wards and corridors of the General Hospital, and found that no less than 23 of them were infected. If it were not for *Culex*, and for men's indifference and apathy, filariasis could be kept down all over the island :—

"There is a perfect water supply, and people can get their water fresh from the standpipes at their doors. Old wells ought to be filled up; no water-barrels or tubs should be allowed, or, if kept, they should be emptied every week or so. Tanks and collections of water in gardens should all be periodically treated with kerosene, or be furnished with closely-fitting covers to prevent mosquitoes getting in. These methods are simple and inexpensive, and each householder should see that they are applied in his garden and grounds. The difficulty begins when one has to take into account the inability of the negro to grasp anything of a hygienic nature. The only way to get over this, would be a system of sanitary inspection by a few competent men. For individual prophylaxis, mosquito-nets ought always to be used; but many, even educated people, still persist in sleeping without them; of course, nothing in this line can be expected of the native population.

"If such means were adopted for Barbadoes, the presence of filarial disease, which at present is quite alarming, could easily, with little trouble and expense, be greatly diminished, and thus save much suffering, as well as loss of time, hideous deformity, and doubtless in not a few instances loss of life."

Thus, in a few years, from experiments on mosquitoes, sparrows, and men, has come the present plan of campaign against malaria, yellow fever, and filariasis; that is, against *Anopheles* and *Culex*. He who would know what is being done to check these diseases in Italy, India, China, Africa, and America, must read Prof. Ross' *Malarial Fever, its Cause, Prevention, and Treatment* (1902), and *Mosquito Brigades, and how to organise them* (1902). There has been nothing like it since Pasteur died. Far and wide, from Staten Island to Cuba, from Hong Kong to Lagos, the work of keeping down the larvæ of *Anopheles* and *Culex* is going on. *Henceforth we have to reckon not with a nameless something, but with a definite parasite, whose conditions of life are known. Before all things, we must shut off the sources of the infection.* For centuries, men had believed in exhalations and miasmata lying all over the land: and, behold, the agents of malaria are in the puddles round a man's house, and the agents of yellow fever are in the water-butt and the broken bottles and old sardine-tins. Science has given the word, and now there are *Anopheles* brigades and *Culex* brigades set going; labourers with brooms and rubbish-carts, sweeping out the stagnant pools, draining the surface soil, and carrying off the odd receptacles that serve to hold mosquito eggs and larvæ. The job, like all sanitary jobs, must be steady, year in, year out: it must be limited to infected places, a whole continent cannot be treated. But there the work is, and will grow; and saves, by unskilled labour, and at a trivial expense, those "non-acclimatised" lives that have hitherto been thrown away as recklessly as the larvæ that are now swept out of the puddles and ditches round African settlements.

XI

PARASITIC DISEASES

The foregoing chapters are concerned with bacteriology alone, and with those curative or preventive methods of treatment that have come out of inoculation-experiments on animals. The lives that are saved, or safeguarded, by these methods, even in one year, must be many thousands in each country of the civilised world. And, beside human lives, there is the protection of sheep and cattle against anthrax, swine against rouget, horses against tetanus, cattle against rinderpest. In Cape Colony alone, so far back as 1899, about half a million cattle had received preventive treatment against rinderpest; and the sum total of human and animal lives saved or safeguarded, in all parts of the world, must be reckoned in millions by this time.

The present chapter, and the next two chapters, are concerned with methods that have come out of experiments on animals, but not out of bacteriology.

It is plain that the grosser parasites of the human body, tapeworms and the like, could not be explained or understood without the help of feeding-experiments on animals. By this method, and by this alone, their life-history was discovered. They were known to Aristotle and to Hippocrates; but nothing was understood about them. They were never studied, for this among other reasons, that men believed in spontaneous

generation; and the presence of lower forms of life inside human bodies was attributed to the fault of the patient, or the work of the devil. Then, at last, Redi (1712), and Swammerdam (1752) in his *Bibel der Natur*, struck at the doctrine of spontaneous generation, saying that it did not apply to insects; and in 1781 Pallas boldly declared that the internal parasites of man came out of eggs, like insects, and not "of themselves." It would be a good theme for an essay—*The paralysing effect, on medicine and surgery, of the doctrine of spontaneous generation.* Rudolphi (1808) and Bremser (1819) opposed Pallas; and von Siebold (1835) and Eschricht (1837) supported him. Then came the great students of this part of biology—Cobbold, Busk, Davaine, van Beneden, Leuckart, Küchenmeister. In 1842, Steenstrup had discovered, in certain insects, the alternation of generations; in 1852, Küchenmeister proved that the generations of internal parasites are similarly alternate. By feeding carnivorous animals with "measly" meat, he produced tapeworms in them; and by feeding herbivorous animals with the ova of tapeworms, he made their muscles "measly."

The feeding of animals was the only possible way to understand the bewildering transformations and transmigrations of the thirty or more entozoa to which flesh is heir. This chapter of pathology makes up in tragedy what it lacks in romance; for these animal parasites have killed whole hosts of people. Take, for instance, the *trichina spiralis*, a minute worm discovered in 1835 encysted in countless numbers in the muscles of the human body; it was studied by Virchow, Leuckart, and others, by feeding-experiments on animals, and was proved to come from infected

half-cooked ham and pork, and to make its way from the alimentary canal all over the body. The name of trichiniasis or trichina-fever was given to the acute illness that came of the sudden dissemination of these myriad parasites into the tissues. Trichiniasis had killed hundreds of people by a most painful death; outbreaks of it, in Germany and elsewhere, had swept through villages like cholera or plague: then Leuckart and Virchow traced it to its source, and it was stopped there—*Above all things, we must shut off the sources of the infection*—the butchers' shops were kept under sanitary inspection, people were warned against half-cooked ham and pork, and there was an end of it.

Or take hydatid disease, which occurs in all parts of the world, and in some countries (Australia, Iceland) is terribly common. The nature of this disease—that it is an animal parasite transmissible between men and dogs—was proved by feeding-experiments on animals. In Iceland, where men and dogs live crowded together in huts, there is an appalling number of deaths from hydatid disease; Leuckart, in 1863, said of it:—

"At present, almost the sixth part of all the inhabitants annually dying in Iceland fall victims to the echinococcus epidemic."

Before Küchenmeister's experiments in 1852, there was no general knowledge of the exact pathology of entozoic disease. The advance was not made by the experimental method alone; other things helped: but among them was neither clinical experience, nor what Sir Charles Bell called "the observation of the just facts of anatomy and of natural motions."

Beside the entozoa, there are also vegetable parasites. Of these, the most important is the *streptothrix actinomyces*, the cause of actinomycosis in man and cattle. Israel, in 1877, gave the first accurate account of it in man; and Böllinger, the same year, studied it in cattle. Ponfick, in 1882, recognised the identity of the disease in man and animals. In 1885, Israel published the collected records of 37 cases in man, tabulated according to the site of the primary infection. Boström, about this time, made cultures of the fungus: but all the earlier attempts at inoculation failed; and it was not till 1891 that Wolff and Israel published their successful inoculations, and thus completed the evidence that actinomycosis is a parasitic infection, a growth of vegetable threads and spores, transmissible between men and animals, and able to keep its vitality outside its host; so that men who are employed with cattle, or have the habit of chewing straws or ears of corn, incur some slight risk of infection. Before 1877, the disease was hardly suspected in man, and was not understood in cattle.

XII

MYXŒDEMA

On 4th October 1873, Sir William Gull read a short paper before the Clinical Society of London, "On a Cretinoid State supervening in Adult Life in Women." This famous first account of myxœdema was based on five cases: it is less than five pages long, it does not suggest a name for the disease, and it says nothing about the thyroid gland. Four years later (23rd October 1877), Dr. Ord read a paper before the Medico-Chirurgical Society of London, "On Myxœdema; a term proposed to be applied to an essential condition in the 'Cretinoid' Affection occasionally observed in Middle-aged Women." His work had begun so far back as 1861; and in this 1877 paper he gave not only clinical observations, but also pathological and chemical facts; and he noted, as one among many changes, wasting of the thyroid gland. He also pointed out the close resemblance between cases of myxœdema and cases of sporadic cretinism.

In 1882, Reverdin stated before the Medical Society of Geneva that signs like those of myxœdema had been observed in some cases of removal of the thyroid gland on account of disease (goître). In April 1883, Kocher of Berne read a paper on this subject, before the Congress of German Surgeons; but he attributed this myxœdema after removal of the gland (cachexia

strumipriva) not directly to the loss of thyroid-tissue, but rather to some sort of interference with free respiration, due to operation. On 23rd November, Sir Felix Semon brought the subject again before the Clinical Society; and on 14th December 1883, the Society appointed a Committee of Investigation to study the whole question.

Their report, 215 pages long, with tabulated records of 119 cases of myxœdema, was published in 1888. It is a monument of good work, historical, clinical, pathological, chemical, and experimental. Twenty years ago, the purpose of the thyroid gland was unknown: a few experiments had been made on it, by Sir Astley Cooper and others, and had failed; and Claude Bernard, in his *Physiologie Opératoire* (published in 1879, soon after his death), makes it clear that nothing was known in his time about it. He is emphasising the fact that anatomy cannot make the discoveries of physiology:—

"The descriptive anatomy, and the microscopic characters, of the thyroid gland, the facts about its blood-vessels and its lymphatics—are not all these as well known in the thyroid gland as in other organs? Is not the same thing true of the thymus gland, and the suprarenal capsules? *Yet we know absolutely nothing about the functions of these organs—we have not so much as an idea what use and importance they may possess—because experiments have told us nothing about them;* and anatomy, left to itself, is absolutely silent on the subject."

Therefore, in 1882–83, things stood at this point—that the removal of a diseased thyroid gland had been followed, in some cases, by a train of symptoms such as Sir William Gull had recorded in 1873. Would the same symptoms follow removal of the healthy gland? The answer was given by Sir Victor Horsley's experi-

ments, begun in 1884. He was able, by removal of the gland, to produce in monkeys a chronic myxœdema, a cretinoid state, the facsimile of the disease in man: the same symptoms, course, tissue-changes, the same physical and mental hebetude, the same alterations of the excretions, the temperature, and the voice. It was now past doubt that myxœdema was due to want of thyroid-tissue, and to that alone; and that "cachexia strumipriva" was due to the loss, by operation, of such remnants of the gland as had not been rendered useless by disease.

The advance had still to be made from pathology to treatment. Here, so far as England is concerned, honour is again due to Sir Victor Horsley. On 8th February 1890, he published the suggestion that thyroid-tissue, from an animal just killed, should be transplanted beneath the skin of a myxœdematous patient:—

"The justification of this procedure rested on the remarkable experiments of Schiff and von Eisselsberg. I only became aware in April 1890, that this proposal had been in fact forestalled in 1889 by Dr. Bircher, in Aarau. (The date of Dr. Bircher's operation was 16th January 1889.) Kocher had tried to do the same thing in 1883, but the graft was soon absorbed; but early in 1889 he tried it again, in five cases, and one greatly improved."

The importance of this treatment, by transplantation of living thyroid-tissue, must be judged by the fact that in 1888 no practical use had yet been made of the scientific work that had been done. The Clinical Society's Report, published that year, gives but half a page to treatment, of the old-fashioned sort; and not a word of hope.

gave him, eleven of them in consultation—*totus medicorum chorus ab omni spe destitutus*—they gave him, as *more generous cardiacs*, the *lapis Goæ*, and the *Bezoar-stone*. The *lapis Goæ* was a dust of topaz, jacinth, sapphire, ruby, pearl, emerald, bezoar, coral, musk, ambergris, and gold, all made into a pill and polished; and the *bezoar* is a calculus found in the intestines of herbivorous animals. Half a century later, the Pharmacopœia of 1721 still included ants' eggs, teeth, *lapis nephriticus*, and other horrors; and in the Pharmacopœia of 1746, though the dust of Egyptian mummies was ruled out, vipers and wood-lice were retained.

Certainly these "last enchantments of the Middle Ages" were slow to depart. Clinical observation, anatomy, and pathology, had all failed to bring about a right understanding of the actions of drugs. It was the physiologists, not the doctors, who first formulated the exact use of drugs; it was Bichat, Magendie, and Claude Bernard. That is the whole meaning of Magendie's work on the upas-poison and on strychnine, and Claude Bernard's work on curari and digitalis. Of these four substances, two only are of any use in practice; yet Magendie's study of strychnine[1] was of immeasurable value, not so much because it gave the doctors a "more generous cardiac," though that was a great gift, but because it revealed the *selective* action of drugs. Contrast his account of strychnine with Ambroise Paré's story how they tested the bezoar-stone on the thief instead of hanging him; contrast Bernard's chapter on curari with Dr. Scarbrugh's notes on the King's death, with

[1] For a full statement of the great value of this study of strychnine, see Cl. Bernard, *Leçons de Physiologie Opératoire*, 1879, p. 89.

all the Crown jewels inside him: you are in two different worlds. The *selective* action of drugs—the affinity between strychnine and the central nerve-cells, between curari and the terminal filaments of the motor nerves—that was the revolutionary teaching of science: and it came, not by experience, but by experiment.

Take Professor Fraser's address on "The Action of Remedies, and the Experimental Method" at the International Medical Congress in London, 1881:—

"The introduction of this method is due to Bichat; and, by its subsequent application by Magendie, pharmacology was originated as the science we now recognise. Bichat represents a transition state, in which metaphysical conceptions were mingled with the results of experience. Magendie more clearly recognised the danger of adopting theories, in the existing imperfections of knowledge; and devoted himself to the supplementing of these imperfections by experiments on living animals. The advantages of such experiments he early illustrated by his investigation on the upas-poison; and afterwards by a research on the then newly-discovered alkaloid, strychnia. . . . He demonstrated the action of this substance upon the spinal cord, by experiments upon the lower animals, so thoroughly, that subsequent investigations have added but little to his results."

Or take Professor Fraser's account of digitalis:—

"It was introduced as a remedy for dropsy; and, on the applications which were made of it for the treatment of that disease, a slowing action upon the cardiac movements was observed, which led to its acquiring the reputation of a cardiac sedative. Numerous observations were made on man by the originators of its application, by Dr. Sanders and many other physicians, in which special attention was paid to its effects upon the circulation; but no further light was thrown upon its remark-

able properties, with the unimportant exception that in some cases it was found to excite the circulation. It was not until the experimental method was applied in its investigation, in the first instance by Claude Bernard, and subsequently by Dybkowsky, Pelikan, Meyer, Boehm, and Schmiedeberg, that the true action of digitalis upon the circulation was discovered. It was shown that the effects upon the circulation were not in any exact sense sedative, but, on the contrary, stimulant and tonic, rendering the action of the heart more powerful, and increasing the tension in the blood-vessels. The indications for its use in disease were thereby revolutionised, and at the same time rendered more exact; and the striking benefits which are now afforded by the use of this substance in most (cardiac) diseases were made available to humanity."

Or take Sir T. Lauder Brunton's account of the action of nitrite of amyl in angina pectoris:—

"The action of nitrite of amyl in causing flushing was first observed by Guthrie, and Sir B. W. Richardson recommended it as a remedy in spasmodic conditions, from the power he thought it to possess of paralysing motor nerves. In the spring of 1867 I had opportunities of constantly observing a patient who suffered from angina pectoris, and of obtaining from him numerous sphygmographic tracings, both during the attack and during the interval. These showed that during the attack the pulse became quicker, the blood-pressure rose, and the arterioles contracted. . . . It seemed probable that the great rise in tension was the cause of the pain, and it occurred to me that if it was possible to diminish the tension by drugs instead of by bleeding, the pain would be removed.

"I knew from unpublished experiments on animals by Dr. A. Gamgee that nitrite of amyl had this power, and therefore tried it on the patient. My expectations were perfectly answered. The pain usually disappeared in three-quarters of a minute after the inhalation began,

and at the same time the pulse became slower and much fuller, and the tension diminished."

Of course it would be easy to lengthen out the list. Aconite, adrenalin, belladonna, calcium chloride, colchicum, cocain, chloral, ergot, morphia, salicylic acid, strophanthus, the chief diuretics, the chief diaphoretics —all these drugs, and many more, have been studied and learned by experiments on animals. Then comes the answer, that drugs act differently on animals and on men. The few instances, that give a wise air to this foolish answer, were known long ago to everybody: they do not so much as touch the facts of daily practice:—

"The action of drugs on man differs from that on the lower animals chiefly in respect to the brain, which is so much more greatly developed in man. Where the structure of an organ or tissue is nearly the same in man and in the lower animals, the action of drugs upon it is similar. Thus we find that carbonic oxide, and nitrites, produce similar changes in the blood of frogs, dogs, and man, that curare paralyses the motor nerves, alike in them all, and veratria exerts upon the muscles of each its peculiar stimulant and paralysing action. Where differences exist in the structure of the various organs, we find, as we would naturally expect, differences in their reaction to drugs. Thus the heart of the frog is simpler than that of dogs or men, and less affected by the central nervous system; we consequently find that while such a drug as digitalis has a somewhat similar action upon the hearts of frogs, dogs, and men, there are certain differences between its effect upon the heart of a frog and on that of mammals.

"Belladonna offers another example of apparent difference in action—a considerable dose of belladonna will produce almost no apparent effect upon a rabbit, while a smaller dose in a dog or a man would cause the rapidity

of the pulse to be nearly doubled. Yet in all three—rabbits, dogs, and men—belladonna paralyses the power of the vagus over the heart. The difference is that in rabbits the vagus normally exerts but little action on the heart, and the effect of its paralysis is consequently slight or hardly appreciable." (Professor Fraser.)

It would be strange indeed, if experts who work in micromillimetres and decimal milligrammes, and study the vanishing-point of microscopic structures, and measure and ordain infinitesimal changes in invisible organisms, were blind to such gross and palpable differences as exist between men and pigeons in their susceptibility to a dose of opium.

Anæsthetics must be reckoned among the drugs that have been studied on animals: but, for the discovery of them, men experimented on themselves. The first use of nitrous oxide (laughing gas) in surgery was 11th December 1844, when Horace Wells, of Connecticut, had it administered to himself for the removal of a tooth. The first use of ether was made by Dr. Long, of Athens, Georgia; but he did not publish the case, or follow up the work: and the honour of the discovery of ether went to Morton, of Boston, who made repeated experiments, both on animals and on himself. The date when he first rendered himself absolutely unconscious for seven or eight minutes, is 30th September 1846; and the first operation under ether was done on 16th October, in the Massachusetts General Hospital. The first use of chloroform was 4th November 1847, that famous evening when Simpson, George Keith, and Matthews Duncan took it together. The whole history of anæsthesia is to be found in the *Practitioner*, Oct. 1896.

It is sometimes said that the men who make experiments on animals ought to make them on themselves.

But they do, hundreds of them, and suffer for it: Heaven knows the list is long enough—the discoverers of anæsthesia, Hunter, Garré, Koch, Klein, Moor, Haffkine, Grassi, Bochefontaine, Quesada, Sanarelli, Pettenkofer—these and hosts more, here or abroad, have done it, as part of the day's work; and some—by accidental infection, like Chabry and Villa, or by deliberate self-inoculation, like Carrion—have been killed:—

"Dr. Angelo Knorr, *Privat-docent* in the Veterinary School of Munich, died on 22nd February from acute glanders, contracted in the course of an experimental research on mallein. Helmann, the Russian investigator who discovered mallein, himself fell a victim to accidental inoculation of the glanders virus. Some time afterwards another Russian, Protopopow, died of glanders contracted in a French laboratory. An Austrian physician, Dr. Koffman-Wellenhof, died of the same disease, contracted in the Institute of Hygiene at Vienna. On 17th January of the present year Dr. Guiseppe Bosso, of the University of Turin, died of infection contracted in the course of cultivations of tubercle-bacilli made in his laboratory. Not long before, Dr Lola, assistant in the maternity department of the Czech University Hospital of Prague, died of tetanus caused by an experimental inoculation made on himself. Some fourteen or fifteen years ago, a medical student of Lima proved that 'verruga Peruana' is an infectious disease by inoculating himself with it, an act of scientific devotion which cost him his life.[1] Besides those who have died, there are many who have only escaped with their lives after long and painful illness. Professor Kourloff contracted anthrax in a laboratory at Munich, and was saved only by vigorous surgery. Dr. Nicolas supplied, in his own person, the first example of tetanus produced in man by inoculation of the pure toxin

[1] Daniel Carrion, born 1859 at Cerro de Pasco, proved, by self-inoculation, the identity of the two forms of the disease, 27th August 1885; died of the disease, 5th October. See *Ann. de l'Inst. Past.*, Sept. 1898.

of the bacillus of Nicolaier." (*Brit. Med. Journal*, 18th March 1899.)

This list is seven years old now; it is twice the length by this time. Typhoid, malaria, yellow fever, have all taken toll of those who study them. It is a long record of the men who fell ill, or died, or killed themselves over their work; and the deaths of Barisch, Dr. Müller, and Nurse Pecha, from plague at Vienna (October 1898) are another instance that there is danger in the constant handling of cultures. But these deaths at Vienna were due to the great carelessness of one man. In laboratories in all parts of the world there are stored cultures of all sorts of organisms, yet no harm comes of it. "More cases of infection occur amongst young medical men attending fever cases, whether in private practice or hospital wards, in a single month, than have occurred in the whole of the laboratories in the world since they were established." (*British Medical Journal*, 29th October 1898.) Outside the laboratory, outside the fever hospitals, the risk is something less than a negligible quantity:—

"Apart from plague and cholera, in all the big laboratories studies are uninterruptedly pursued, from one end of the year to the other, upon anthrax, glanders, influenza, Malta fever, various tropical diseases which do not exist at all or are rare in the countries where they are being studied. The laboratories in question are situated in the largest and most important towns of their respective countries; and, within those towns, very often in the most fashionable or most populous centres. . . . On no occasion was there even a suspicion aroused of an epidemic having been produced by any of the above-mentioned institutes, or by those tens of thousands of operations against cholera performed in India." (Haffkine, *Madras Mail*, 8th December 1898.)

XIV

SNAKE-VENOM

THE Report of the 1875 Commission said :—

"It is not possible for us to recommend that the Indian Government should be prohibited from pursuing its endeavours to discover an antidote for snake-bites; or that, without such an effort, your Majesty's Indian subjects should be left to perish in large numbers annually from the effects of these poisons."

Certainly it was not possible; and the numbers are large indeed. During 1897, 4227 persons were killed by wild animals in India, and 20,959 by snakes. (*British Medical Journal*, 5th November 1898.)

Sir Joseph Fayrer's name must be put in the highest place of all those who have studied the venomous snakes of India.

Sewell, in 1887, showed that animals could be rendered immune, by repeated inoculation with minute quantities of rattlesnake-venom, to a dose seven times as large as would kill an unprotected animal. Kanthack, in 1891, immunised animals in the same way against cobra-venom. He also made experiments to ascertain whether the blood-serum of these animals acted as an antidote to the venom. Then came the work of Calmette, Fraser, Phisalix, Bertrand, Martin (Australia), Stephens, and Meyers. Professor Fraser's observations on the antidotal properties of the bile are,

of course, of the utmost importance; not only in preventive medicine, but also in physiology. The results obtained by Calmette are a good instance of the fineness and accuracy of the experimental method. It is to be noted that the animals were inoculated with a fine needle, not thrust into cages with snakes, as at zoological gardens; and that an animal thus poisoned has a painless death. The different venoms were measured in decimal milligrammes, and their potency was estimated according to the body-weight of the animal inoculated. As with tetanus, so with snake-venom, there must be a standard, or "unit of toxicity."

"The following table gives the relative toxicity, for 1 kilogr. of rabbit, of the different venoms that I have tested. To denote this toxicity I use terms such as Behring, Roux, and Vaillard used for the toxin of tetanus, taking the number of grammes of animal killed by one gramme of toxin:—

1. Venom of *naja* . . .	0.25 mgr. per kilogr. of rabbit. One gramme of this venom kills 4000 kilogrammes of rabbit; it has, therefore, an activity of	4,000,000
2. Venom of *hoplocephalus*	0.29 mgr. . .	3,450,000
3. Venom of *pseudechis*	1.25 mgr. . .	800,000
4. Venom of *pelias berus*	4.00 mgr. . .	250,000

"Of course, this estimation of virulence is not absolute; it varies considerably according to the species of animal tested. Thus the guinea-pig, and still more the rat, are extremely sensitive. For instance, 0.15 mgr. of viper-venom is enough to kill, in less than 12 hours, 500 grammes of guinea-pig; so that the activity of this venom with a guinea-pig is 3,333,000, but with a rabbit is not more than 650,000. With more resistant animals, the opposite result is obtained; about 10 mgr. of cobra-venom are necessary to kill a dog of 6.50 kilogrm. weight; but to kill the same weight of rabbit 1.65 mgr. is enough. Thus the virulence of this venom with the rabbit is 4,000,000; but with the dog not more than 650,000."

By experiments in test-tubes, Calmette studied these venoms under the influences of heat and various chemical agents. He found how to attenuate their virulence, and how to diminish the local inflammation round the point of inoculation; and it was in the course of these test-tube experiments and inoculations that he discovered the value of calcium hypochlorite as a local application. Working, by various methods, with attenuated venoms, he was able to immunise animals:—

"I have come to immunise rabbits against quantities of venom that are truly colossal. I have got several, vaccinated more than a year ago, which take, without the least discomfort, so much as 40 mgr. of venom of *naja tripudians* at a single injection; that is to say, enough to kill 80 rabbits of 2 kilogr. weight, or 5 dogs.

"Five drops of serum from these rabbits wholly neutralise *in vitro* (in a glass test-tube) the toxicity of 1 mgr. of *naja*-venom."

By 1894 he had found that the serum of an animal, thus immunised by graduated doses of one kind of venom, neutralised other kinds of venom:—

"If 1 mgr. of cobra-venom, or 4 mgr. of viper-venom, be mixed, in a test-tube, with a small quantity of serum from an immunised rabbit, and a fresh rabbit be inoculated with this mixture, it does not suffer any discomfort. It is not even necessary that the serum should come from an animal vaccinated against the same sort of venom as that in the mixture. *The serum of a rabbit immunised against the venom of the cobra or the viper acts indifferently on all the venoms that I have tested.*"

In 1894 he had prepared enough serum for the treatment to be tried by his own countrymen practising in some of the French colonies. In April 1895, he gave the following account of his work:—

"I have immunised two asses, one having received 220 mgr. of *naja*-venom from 25th September to 31st December 1894, and the other 160 mgr. from 15th October to 31st December. The serum of the first of these two animals has now reached this point, that half a cubic centimetre destroys the toxicity of 1 mgr. of *naja*-venom. Four cubic centimetres of this serum, injected four hours before the inoculation of a dose of venom enough to kill twice over, preserve the animal in every case. It is also therapeutic, under the conditions that I have already defined; that is to say, if you first inoculate a rabbit with such a dose of venom as kills the control-animals in three hours, and then, an hour after injecting the venom, inject under the skin of the abdomen 4 to 5 cubic centimetres of serum, recovery is the rule. When you interfere later than this the results are uncertain; and in all my experiments the delay of an hour and a half is the most that I have been able to reach.

"This antivenomous serum of asses has these same antitoxic properties with all kinds of snake-venom; it is equally active *in vitro*, preventive, and therapeutic, with the venoms of *cerastes*, of *trigonocephalus*, of *crotalus*, and of four kinds of Australian snakes that Mr. MacGarvie Smith has sent to M. Roux. I am still injecting these two animals with venom, and I hope to give to their serum at last a much greater antitoxic power."

In 1896 four successful cases of this treatment in the human subject were reported in the *British Medical Journal*. In 1898 Calmette made the following statement of his results:—

"It is now nearly two years since the use of my antivenomous serum was introduced in India, in Algeria, in Egypt, on the West Coast of Africa, in America, in the West Indies, Antilles, &c. It has been very often used for men and domestic animals (dogs, horses, oxen), and up to now none of those that have received an injection of serum have succumbed. . . . A great number of

observations have been communicated to me, and not one of them refers to a case of failure." (*British Medical Journal*, 14th May 1898.)

Good accounts of Fraser's and Calmette's work are given by Dr. Stone in the *Boston Medical and Surgical Journal*, 7th April 1898, and by Staff-Surgeon Andrews, R.N., in the *British Medical Journal*, 9th September 1899. For other cases see the *Pioneer*, 10th August 1899, the *Lancet*, 25th November 1899, and the *British Medical Journal*, 23rd December 1899. In one of these cases, recorded by Dr. Rennie, the patient was, literally, at the point of death, but recovered after the serum had been injected. Two cases have also been recorded of cobra-bite during work in the laboratory: both of them recovered after injection. "Every Government or private dispensary," says Surgeon Beveridge, "should be supplied with antivenene, which is certainly the best remedy for snake-bite available." The cases are few at present; but it does not appear that the treatment has failed in any case; and, with a new remedy of this kind, it is fairly certain that failures would be published.

From all these instances in physiology, pathology, bacteriology, and therapeutics, we come to consider the Act relating to experiments on animals in the United Kingdom. Many subjects have been left out; among them, the work of the last few years on the suprarenal glands and adrenalin, and Dr. William Hunter's admirable work on pernicious anæmia. No attempt has been made to describe the researches of experts in many countries into the nature of malignant disease, or to guess what may come of the discovery that mice can be immunised against that form of cancer which occurs

in mice and is inoculable from mouse to mouse. Nothing has been said of the discovery that the African sleeping-sickness is due to a blood-parasite carried by flies from man to man. Nothing has been said about those discoveries in bacteriology that have not yet been applied to practice, or of the many inventions of medical and surgical practice that owe only an indirect debt to experiments on animals. Artificial respiration, the transfusion of saline fluid, the hypodermic administration of drugs, the use of oxygen for inhalation, the torsion of arteries, the grafting of skin, the transplantation of bone, the absorbable ligature, the diagnostic and therapeutic uses of electricity, the rational employment of blood-letting —all these good methods have been left out of the list; only some facts have been presented, those that mark most clearly the advance of knowledge and of practice, and stand up even above the rest of the work. There they will stand, when we are all dead and gone: and by them, as by landmarks, all further advance will be guided.



PART III

THE ACT RELATING TO EXPERIMENTS ON ANIMALS IN GREAT BRITAIN AND IRELAND

ACT 39 AND 40 VIC. c. 77

THE Royal Commission "On the Practice of subjecting Live Animals to Experiments for Scientific Purposes," was appointed on 22nd June 1875. Its members were —Lord Cardwell (chairman), Lord Winmarleigh, Mr. W. E. Forster, Sir John Karslake, Mr. Huxley, Mr. (Sir John) Erichsen, and Mr. Hutton. Between 5th July and 30th December, 53 witnesses were examined, and 6551 questions were put and answered. The report of the Commission bears date 8th January 1876, and in that year the present Act received the Royal Assent.

The evidence before the Commission was all, or nearly all, concerned with physiology, with the work of Magendie, Claude Bernard, and Sir Charles Bell, the action of curare, the *Handbook of the Physiological Laboratory*, the teaching of physiology, and so forth. Very little was said of pathology; and of bacteriology next to nothing. Practically, physiology alone came before the Commissioners; and such experiments in physiology as are now, the youngest of them, more than thirty years old.

Bacteriology, at the time of the passing of the Act, had hardly made a beginning. Therefore the Act made no special provision for inoculations, injections, and the whole study of immunisation of animals and men against disease. Experiments of this kind have to be scheduled under one of the existing certificates, to bring them under an Act that was drafted without fore-

knowledge of them. Certificate A or Certificate B has to be used for this purpose :—

Certificate A.

"We hereby certify that, in our opinion, insensibility in the animal on which any such experiment may be performed cannot be produced by anæsthetics without necessarily frustrating the object of such experiment."

Certificate B.

"We hereby certify that, in our opinion, the killing of the animal on which any such experiment is performed before it recovers from the influence of the anæsthetic administered to it, would necessarily frustrate the object of such experiment."

Under one or other of these certificates must be scheduled all inoculations, injections, feeding-experiments, transplantations of particles of disease, immunisations, and the like. They must be scheduled somehow ; and that is the only way of doing it. Where the act of inducing the disease would itself give any pain, if an anæsthetic were not administered —as in the subdural inoculation of a rabbit, or the intra-peritoneal inoculation of an animal with a particle of cancerous tissue—there the licensee must hold, together with the license, Certificate B, because the act of inducing the disease is itself an operation, done under an anæsthetic. If the animal be a dog or a cat, he must hold Certificates B and EE ; if it be a horse, ass, or mule, Certificates B and F.

Where the act of inducing the disease is not itself painful—as in ordinary inoculation, and in feeding-experiments—the licensee must hold, together with his

license, Certificate A, because the animal is not anæsthetised. It is not a painful operation ; the experiment consists not in the act of putting the hypodermic needle under the animal's skin, but in the subsequent observation of the course of the disease. Take, for instance, the inoculation of a guinea-pig with tubercle-bacilli : the experiment is the production of tubercle ; the experiment lasts till the animal is killed and found to be infected ; it is therefore scheduled under Certificate A. Or take the testing, on an animal, of an antitoxin ; the experiment is not the injection, but the observation of the result ; the animal may not suffer, but the injection must still be done under Certificate A. And, if the animal be a dog or a cat, the licensee must hold Certificates A and E ; or, if it be a horse, ass, or mule, Certificates A and F.

This want of a special certificate for inoculations is an important matter, because it has led to the belief that painful operations are performed, without anæsthesia, in cases where the only instrument used is a needle. It is hardly reasonable, for instance, that the inoculation of a mouse should be scheduled as a painful operation performed without anæsthesia. The disease, thus painlessly induced, may in many cases be called painless ; for instance, snake-venom in the rat, septicæmia in the mouse, malaria in small birds. In other cases, there are such pain and fever as are part of the disease. The form that rabies take in rabbits may fairly be called painless. Inoculations not under the skin, but into the anterior chamber of the eye, are very seldom made ; they sound cruel, but cocain renders the surface of the eye wholly insensitive, and the anterior chamber is so far insensitive that a man with blood or pus (*hypopyon*) in the anterior chamber of the eye may

guilty of an offence against this Act, and shall, if it be the first offence, be liable to a penalty not exceeding fifty pounds, and if it be the second or any subsequent offence, be liable, at the discretion of the court by which he is tried, to a penalty not exceeding one hundred pounds, or to imprisonment for a period not exceeding three months.

3. The following restrictions are imposed by this Act with respect to the performance on any living animal of an experiment calculated to give pain; that is to say,

(1.) The experiment must be performed with a view to the advancement by new discovery of physiological knowledge or of knowledge which will be useful for saving or prolonging life or alleviating suffering; and

(2.) The experiment must be performed by a person holding such license from one of Her Majesty's Principal Secretaries of State, in this Act referred to as the Secretary of State, as is in this Act mentioned, and in the case of a person holding such conditional license as is hereinafter mentioned, or of experiments performed for the purpose of instruction in a registered place; and

(3.) The animal must, during the whole of the experiment, be under the influence of some anæsthetic of sufficient power to prevent the animal feeling pain; and

(4.) The animal must, if the pain is likely to continue after the effect of the anæsthetic has ceased, or if any serious injury has been inflicted on the animal, be killed before it recovers from the influence of the anæsthetic which has been administered; and

(5.) The experiment shall not be performed as an illustration of lectures in medical schools, hospitals, colleges, or elsewhere; and

(6.) The experiment shall not be performed for the purpose of attaining manual skill.

Provided as follows; that is to say,

(1.) Experiments may be performed under the foregoing provisions as to the use of anæsthetics by a person giving illustrations of lectures in medical schools, hospitals, or colleges, or elsewhere, on such certificate being given as in this Act mentioned, that the proposed experiments are absolutely necessary for the due instruction of the persons to whom such lectures are given with a view to their acquiring physiological knowledge, or knowledge which will be useful to them for saving or prolonging life, or alleviating suffering; and

(2.) Experiments may be performed without anæsthetics on such certificate being given as in this Act mentioned, that insensibility cannot be produced without necessarily frustrating the object of such experiments; and

(3.) Experiments may be performed without the person who performed such experiments being under an obligation to cause the animal, on which any such experiment is performed, to be killed before it recovers from the influence of the anæsthetic, on such certificate being given as in this Act mentioned, that the so killing the animal would necessarily frustrate the object of the experiment, and provided that the animal be killed as soon as such object has been attained; and

(4.) Experiments may be performed not directly for the advancement by new discovery of physiological knowledge, or of knowledge which will be useful for saving or prolonging life, or alleviating suffering, but for the purpose of testing a particular former discovery alleged to have been made for the advancement of such knowledge as last aforesaid, on such certificate being given as is in this Act mentioned that such

testing is absolutely necessary for the effectual advancement of such knowledge.

4. The substance known as urari or curare shall not for the purposes of this Act be deemed to be an anæsthetic.

5. Notwithstanding anything in this Act contained, an experiment calculated to give pain shall not be performed without anæsthetics on a dog or cat, except on such certificate being given as in this Act mentioned, stating, in addition to the statements hereinbefore required to be made in such certificate, that for reasons specified in the certificate the object of the experiment will be necessarily frustrated unless it is performed on an animal similar in constitution and habits to a cat or dog, and no other animal is available for such experiment; and an experiment calculated to give pain shall not be performed on any horse, ass, or mule except on such certificate being given as in this Act mentioned that the object of the experiment will be necessarily frustrated unless it is performed on a horse, ass, or mule, and that no other animal is available for such experiment.

6. Any exhibition to the general public, whether admitted on payment of money or gratuitously, of experiments on living animals calculated to give pain shall be illegal.

Any person performing or aiding in performing such experiments shall be deemed to be guilty of an offence against this Act, and shall, if it be the first offence, be liable to a penalty not exceeding fifty pounds, and if it be the second or any subsequent offence, be liable, at the discretion of the court by which he is tried, to a penalty not exceeding one hundred pounds, or to imprisonment for a period not exceeding three months.

And any person publishing any notice of any such intended exhibition by advertisement in a newspaper, placard, or otherwise shall be liable to a penalty not exceeding one pound.

A person punished for an offence under this section shall not for the same offence be punishable under any other section of this Act.

Administration of Law

7. The Secretary of State may insert, as a condition of granting any license, a provision in such license that the place in which any experiment is to be performed by the licensee is to be registered in such manner as the Secretary of State may from time to time by any general or special order direct; provided that every place for the performance of experiments for the purpose of instruction under this Act shall be approved by the Secretary of State, and shall be registered in such manner as he may from time to time by any general or special order direct.

8. The Secretary of State may license any person whom he may think qualified to hold a license to perform experiments under this Act. A license granted by him may be for such time as he may think fit, and may be revoked by him on his being satisfied that such license ought to be revoked. There may be annexed to such license any conditions which the Secretary of State may think expedient for the purpose of better carrying into effect the objects of this Act, but not inconsistent with the provisions thereof.

9. The Secretary of State may direct any person performing experiments under this Act from time to time to make such reports to him of the result of such experiments, in such form and with such details as he may require.

10. The Secretary of State shall cause all registered places to be from time to time visited by inspectors for the purpose of securing a compliance with the provisions of this Act, and the Secretary of State may, with the assent of the Treasury as to number, appoint any special inspectors, or may from time to time assign the duties of

any such inspectors to such officers in the employment of the Government, who may be willing to accept the same, as he may think fit, either permanently or temporarily.

11. Any application for a license under this Act and a certificate given as in this Act mentioned must be signed by one or more of the following persons; that is to say,

The President of the Royal Society;

The President of the Royal Society of Edinburgh;

The President of Royal Irish Academy;

The Presidents of the Royal Colleges of Surgeons in London, Edinburgh, or Dublin;

The Presidents of the Royal Colleges of Physicians in London, Edinburgh, or Dublin;

The President of the General Medical Council;

The President of the Faculty of Physicians and Surgeons of Glasgow;

The President of the Royal College of Veterinary Surgeons, or the President of the Royal Veterinary College, London, but in the case only of an experiment to be performed under anæsthetics with a view to the advancement by new discovery of veterinary science;

and also (unless the applicant be a professor of physiology, medicine, anatomy, medical jurisprudence, materia medica, or surgery in a university in Great Britain or Ireland, or in University College, London, or in a college in Great Britain or Ireland, incorporated by royal charter) by a professor of physiology, medicine, anatomy, medical jurisprudence, materia medica, or surgery in a university in Great Britain or Ireland, or in University College, London, or in a college in Great Britain or Ireland, incorporated by royal charter.

Provided that where any person applying for a certificate under this Act is himself one of the persons authorised to sign such certificate, the signature of some other of such persons shall be substituted for the signature of the applicant.

A certificate under this section may be given for such

time or for such series of experiments as the person or persons signing the certificate may think expedient.

A copy of any certificate under this section shall be forwarded by the applicant to the Secretary of State, but shall not be available until one week after a copy has been so forwarded.

The Secretary of State may at any time disallow or suspend any certificate given under this section.

12. The powers conferred by this Act of granting a license or giving a certificate for the performance of experiments on living animals may be exercised by an order in writing under the hand of any judge of the High Court of Justice in England, of the High Court of Session in Scotland, or of any of the superior courts in Ireland, including any court to which the jurisdiction of such last-mentioned courts may be transferred, in a case where such judge is satisfied that it is essential for the purposes of justice in a criminal case to make any such experiment.

Legal Proceedings

13. A justice of the peace, on information on oath that there is reasonable ground to believe that experiments in contravention of this Act are being performed by an unlicensed person in any place not registered under this Act, may issue his warrant authorising any officer or constable of police to enter and search such place, and to take the names and addresses of the persons found therein.

Any person who refuses admission on demand to a police officer or constable so authorised, or obstructs such officer or constable in the execution of his duty under this section, or who refuses on demand to disclose his name or address, or gives a false name or address, shall be liable to a penalty not exceeding five pounds.

14. In England, offences against this Act may be prosecuted and penalties under this Act recovered before a court of summary jurisdiction in manner directed by the Summary Jurisdiction Act.

In England "Summary Jurisdiction Act" means the Act of the session of the eleventh and twelfth years of the reign of Her present Majesty, chapter forty-three, intituled "An Act to facilitate the performance of the duties of justices of the peace "out of sessions within England and Wales with "respect to summary convictions and orders," and any Act amending the same.

"Court of summary jurisdiction" means and includes any justice or justices of the peace, metropolitan police magistrate, stipendiary or other magistrate, or officer, by whatever name called, exercising jurisdiction in pursuance of the Summary Jurisdiction Act: Provided that the court when hearing and determining an information under this Act shall be constituted either of two or more justices of the peace in petty sessions, sitting at a place appointed for holding petty sessions, or of some magistrate or officer sitting alone or with others at some court or other place appointed for the administration of justice, and for the time being empowered by law to do alone any act authorised to be done by more than one justice of the peace.

15. In England, where a person is accused before a court of summary jurisdiction of any offence against this Act in respect of which a penalty of more than five pounds can be imposed, the accused may, on appearing before the court of summary jurisdiction, declare that he objects to being tried for such offence by a court of summary jurisdiction, and thereupon the court of summary jurisdiction may deal with the case in all respects as if the accused were charged with an indictable offence and not an offence punishable on summary conviction, and the offence may be prosecuted on indictment accordingly.

16. In England, if any party thinks himself aggrieved by any conviction made by a court of summary jurisdic-

tion on determining any information under this Act, the party so aggrieved may appeal therefrom, subject to the conditions and regulations following:—

(1.) The appeal shall be made to the next court of general or quarter sessions for the county or place in which the cause of appeal has arisen, holden not less than twenty-one days after the decision of the court from which the appeal is made; and

(2.) The appellant shall, within ten days after the cause of appeal has arisen, give notice to the other party and to the court of summary jurisdiction of his intention to appeal, and of the ground thereof; and

(3.) The appellant shall, within three days after such notice, enter into a recognizance before a justice of the peace, with two sufficient sureties, conditioned personally to try such appeal, and to abide the judgment of the court thereon, and to pay such costs as may be awarded by the court, or give such other security by deposit of money or otherwise as the justice may allow; and

(4.) Where the appellant is in custody the justice may, if he think fit, on the appellant entering into such recognizance or giving such other security as aforesaid, release him from custody; and

(5.) The court of appeal may adjourn the appeal, and upon the hearing thereof they may confirm, reverse, or modify the decision of the court of summary jurisdiction, or remit the matter to the court of summary jurisdiction with the opinion of the court of appeal thereon, or make such other order in the matter as the court thinks just, and if the matter be remitted to the court of summary jurisdiction the said last-mentioned court shall thereupon re-hear and decide the information in accordance with the order of the

said court of appeal. The court of appeal may also make such order as to costs to be paid by either party as the court thinks just.

17. In Scotland, offences against this Act may be prosecuted and penalties under this Act recovered under the provisions of the Summary Procedure Act, 1864, or if a person accused of any offence against this Act in respect of which a penalty of more than five pounds can be imposed, on appearing before a court of summary jurisdiction, declare that he objects to being tried for such offence in the court of summary jurisdiction, proceedings may be taken against him on indictment in the Court of Justiciary in Edinburgh or on circuit.

Every person found liable in any penalty or costs shall be liable in default of immediate payment to imprisonment for a term not exceeding three months, or until such penalty or costs are sooner paid.

18. In Ireland, offences against this Act may be prosecuted and penalties under this Act recovered in a summary manner, subject and according to the provisions with respect to the prosecution of offences, the recovery of penalties, and to appeal of the Petty Sessions (Ireland) Act, 1851, and any Act amending the same, and in Dublin of the Acts regulating the powers of justices of the peace or of the police of Dublin metropolis. All penalties recovered under this Act shall be applied in manner directed by the Fines (Ireland) Act, 1871, and any Act amending the same.

19. In Ireland, where a person is accused before a court of summary jurisdiction of any offence against this Act in respect of which a penalty of more than five pounds can be imposed, the accused may, on appearing before the court of summary jurisdiction, declare that he objects to being tried for such offence by a court of summary jurisdiction, and thereupon the court of summary jurisdiction may deal with the case in all respects as if the accused were charged with an in-

dictable offence and not an offence punishable on summary conviction, and the offence may be prosecuted on indictment accordingly.

20. In the application of this Act to Ireland the term "the Secretary of State" shall be construed to mean the Chief Secretary to the Lord Lieutenant of Ireland for the time being.

21. A prosecution under this Act against a licensed person shall not be instituted except with the assent in writing of the Secretary of State.

22. This Act shall not apply to invertebrate animals.

II.—Anæsthetics under the Act

In almost every case, the anæsthetic used is chloroform or ether; sometimes it is combined with or followed by morphia or chloral. The nature of the anæsthetic used in each case must, of course, be stated in the returns sent to the Home Office.

Of the use of ether, it need only be said that animals take it well, and that there is no difficulty in rendering them unconscious with it. With some animals, chloroform is equally good. Professor Hobday, of the Royal Veterinary College, published in 1898 an account of 500 administrations of chloroform to dogs, for operations, with only one death. Still, for dogs and cats, ether is used in preference to chloroform. Other animals take chloroform well.

Morphia is seldom used alone; but, in some cases, it is used after chloroform or ether. That morphia is a "real anæsthetic" is certain, for there are deaths every year from an over-dose of it. Again, it is certain that an animal, so far under the influence of morphia that it lies still, cannot be suffering, for the drug does not act

directly on the muscles but on the higher nervous centres.

Very rarely a dog may fail to come readily under the influence of morphia, may be excited by it, not narcotized. But this is altogether exceptional. An animal in such a condition would not be suited for experiment, and another anæsthetic would be given. Except in these rare cases, animals take morphia well and are profoundly influenced by it.

Curare is not an anæsthetic under the Act. It is illegal to use it as an anæsthetic. In this country it is seldom used at all, and it is never used alone in any experiment involving any sort or kind of painful operation. In every such case a recognised anæsthetic must be given, and is given.[1]

A good account of curare was published in the *Edinburgh Review*, July 1899.

"The Act of 1876 expressly forbids its use as an anæsthetic. When it is used, it must be supplemented with some other drug to relieve pain. A good deal of misconception exists as to the actual physiological effect of curare. Claude Bernard believed that it did not in any way affect the sensory nerves, and he described in theatrical terms the animal as being unable to stir, but suffering horrible torture. It is pretty certainly known now that Claude Bernard was wrong, and that, though curare acts first upon the motor nerves, it also, though less rapidly, paralyses the sensory nerves. . . . Probably the truth is, that, like all other nerve-poisons, the effect of curare varies with the dose. The muscular nerves are the first affected, then the sensory, and finally the central nervous system. As a matter of fact, however, morphia or some other narcotic is always given in addition to curare when it is used in laboratory work in England."

[1] See Part IV., "Curare."

III.—Latest Report (1905) of Inspectors under the Act

(The various tables of names, places, &c., and the references to them, which are contained in this Report, need not be reprinted here. The Report, and other papers relating to the Act, may be bought for a few pence from Wyman & Sons, Ltd., Fetter Lane, E.C.)

ENGLAND AND SCOTLAND

April 17th, 1906.

Sir,—I have the honour to submit the following Report on Experiments performed in England and Scotland during the Year 1905, under the Act 39 & 40 Vict. c. 77. . . . Six new places were registered for the performance of experiments, and one place was removed from the register during the year. All licensees were restricted to the registered place or places specified on their licenses, with the exception of those who were permitted to perform inoculation experiments in places other than a "registered place," with the object of studying outbreaks of disease occurring in remote districts or under circumstances which render it impracticable to perform the experiment in a "registered place."

The total number of licensees was 381. Reports have been furnished by (or, in a few exceptional cases, on behalf of) these licensees in the form required by the Secretary for State. The reports show that 122 licensees performed no experiments. The numbers given above include 22 licensees whose licences expired on February 28, 1905, and who returned no experiments in 1905.

Tables I., II., and III. afford evidence,—

1. That licences and certificates have been granted and allowed only upon the recommendation of persons of high scientific standing;

2. That the licensees are persons who, by their training and education, are fitted to undertake experimental work and to profit by it;

3. That all experimental work has been conducted in suitable places.

Table IV. shows the number and the nature of the experiments returned by each licensee mentioned in Table II., specifying whether these experiments were done under the licence alone or under any special certificate.

Table IV. is divided into two parts, A. and B., for the purpose of separating experiments which were performed without anæsthetics from experiments in which anæsthetics were used.

The total number of experiments included in Table IV. (A.) is 2506.

Of these there were performed,—

Under Licence alone[1]	1348
„ Certificate C.	145
„ Certificate B.	665
„ Certificate B.+EE. . . .	346
„ Certificate B.+F. . . .	2

[1] In experiments performed under licence alone, the animal must during the whole of the experiment be under the influence of some anæsthetic of sufficient power to prevent the animal feeling pain; and the animal must, if the pain is likely to continue after the effect of the anæsthetic has ceased, or if any serious injury has been inflicted on the animal, be killed before it recovers from the influence of the anæsthetic which has been administered.

Certificate C. allows experiments to be performed, under the foregoing provisions as to the use of anæsthetics, in illustration of lectures.

Certificate B. exempts the person performing the experiment from the obligation to cause the animal on which the experiment is performed to be killed before it recovers from the influence of the anæsthetic; and when the animal is a dog or a cat, Certificate EE. is also necessary.

Certificate A. allows experiments to be performed without anæsthetics; and when the animal on which the experiment is performed is a dog or a cat, Certificate E. is also necessary.

Certificate F. is required in all cases of experiments on a horse, ass, or mule.

Table IV. (B.) is devoted entirely to inoculations, hypodermic injections, and some few other proceedings, performed without anæsthetics. It includes 35,429 experiments, whereof there were performed,—

Under Certificate A.	34,778
„ Certificate A.+E. . . .	549
„ Certificate A.+F. . . .	102

The total number of experiments is 37,935, being 5373 more than in 1904; the increase in the number of experiments included in Table IV. (A.) is 290, and in Table IV. (B.), 5083.

All experiments involving a serious operation are placed in Table IV. (A.). The larger part of the experiments included in this Table, viz., all performed under licence alone, and under Certificate C., 1493 in number, come under the provision of the Act that the animal must be kept under an anæsthetic during the whole of the experiment, and must, if the pain is likely to continue after the effect of the anæsthetic has ceased, or if any serious injury has been inflicted on the animal, be killed before it recovers from the influence of the anæsthetic.

In the experiments performed under Certificate B., or B. linked with EE. or with F., 1013 in number, the initial operations are performed under anæsthetics, from the influence of which the animals are allowed to recover. The operations are required to be performed antiseptically, so that the healing of the wounds shall, as far as possible, take place without pain. If the antiseptic precautions fail, and suppuration occurs, the animal is required to be killed. It is generally essential for the success of these experiments that the wounds should heal cleanly, and the surrounding parts remain in a healthy condition. After the healing of the wounds the animals are not necessarily, or even generally, in pain, since experiments involving the removal of important organs, including portions of the brain, may be performed without giving rise to pain after the recovery

from the operation; and after the section of a part of the nervous system, the resulting degenerative changes are painless.

In the event of a subsequent operation being necessary in an experiment performed under Certificate B., or B. linked with EE. or with F., a condition is attached to the licence requiring all operative procedures to be carried out under anæsthetics of sufficient power to prevent the animal feeling pain; and no observations or stimulations of a character to cause pain are allowed to be made without the animals being anæsthetised.

In no case has a cutting operation more severe than a superficial venesection been allowed to be performed without anæsthetics.

The experiments included in Table IV. (B.), 35,429 in number, are all performed without anæsthetics. They are mostly inoculations, but a few are feeding experiments, or the administration of various substances by the mouth, or the abstraction of a minute quantity of blood for examination. In no instance has a certificate dispensing with the use of anæsthetics been allowed for an experiment involving a serious operation. Inoculations into deep parts, involving a preliminary incision in order to expose the part into which the inoculation is to be made, are required to be performed under anæsthetics, and are therefore placed in Table IV. (A.).

It will be seen that the operative procedures in experiments performed under Certificate A., without anæsthetics, are only such as are attended by no considerable, if appreciable, pain. The Certificate is, in fact, not required to cover these proceedings, but to allow of the subsequent course of the experiment. The experiment lasts during the whole period from the administration of the drug, or injection, until the animal recovers from the effects, if any, or dies, or is killed, possibly extending over several days, or even weeks. The substance administered may give rise to poisoning, or set up a condition of disease, either of which may lead to a fatal termination. To administer to an animal such a poison as diphtheria toxin,

for example, or to induce such a disease as tuberculosis, although it may not be accompanied by acute suffering, is held to be a proceeding "calculated to give pain," and therefore experiments of the kind referred to come within the scope of the Act 39 & 40 Vict., c. 77. The Act provides that, unless a special certificate be obtained, the animal must be kept under an anæsthetic during the whole of the experiment; and it is to allow the animal to be kept without an anæsthetic during the time required for the development of the results of the administration that Certificate A. is given and allowed in these cases.

It must not be assumed that the animal is in pain during the whole of this time. In cases of prolonged action of an injected substance, even when ending fatally, the animal is generally apparently well, and takes its food as usual, until a short time before death. The state of illness may last only a very few hours, and in some cases it is not observed at all.

In a very large number of the experiments included in Table IV. (B.), the results are negative, and the animals suffer no inconvenience whatever from the inoculation. These experiments are therefore entirely painless.

In the event of pain ensuing as the result of an inoculation, a condition attached to the licence requires that the animal shall be killed under anæsthetics as soon as the main result of the experiment has been attained.

The number of inoculations and similar proceedings recorded in Table IV. (B.) continues to increase in accordance with the progressive importance attached to biological tests generally in practical medicine for the diagnosis, treatment and prevention of disease, and to the more widely recognised need for such experiments on the part of those responsible for the care of the public health. Several County Councils and Municipal Corporations have their own laboratories in which bacteriological investigations are carried on, including the necessary tests on living animals; and many others have arrangements by which similar observations are made on their behalf in the laboratories of Universities, Colleges, and other In-

stitutions. A sewage farm is registered as a place in which experiments on living animals may be performed in order that the character of the effluent may be tested by its effects on the health of fish. The Board of Agriculture has two laboratories which are registered for the performance of experiments having for their object the detection and study of the diseases of animals. In other places experiments have been made on behalf of the Home Office, the War Office, the India Office, the Local Government Board, the Office of Works, the Board of Agriculture and Fisheries, and the Metropolitan Asylums Board. A very large proportion of the experiments in Table IV. (B.) have thus been performed either on behalf of Official Bodies with a view to the preservation of the public health, or directly for the diagnosis and treatment of disease. Forty-one licensees return over 8000 experiments which were performed for Government Departments, County Councils, or Municipal Corporations; 2187 experiments were made by four licensees for the Royal Commission on Tuberculosis; twelve licensees performed 6265 experiments, almost all inoculations, for testing antitoxic sera and vaccines and standardising drugs; and 12,187 experiments, mostly inoculations into mice, were performed on behalf of the Imperial Cancer Research Fund.

The number of injections made during the year 1905 for the diagnosis of rabies in dogs is 27; these are placed in Table IV. (A.).

During the year the usual inspections of registered places have been made by Sir James Russell, by myself, and by Mr. W. B. L. Trotter, who was appointed temporary Assistant Inspector during my absence for three months. We have found the animals suitably lodged and well cared for, and the licensees attentive to the requirements of the Act, as well as to the conditions appended to their licences by the Secretary of State.

The irregularities recorded during the year have been few, and not of a serious character.

Two licensees, holding certificates (A.) entitling them to perform inoculations without anæsthetics, administered an anæsthetic during some of their experiments, whereas the Act prescribes another form of certificate (B.) when an animal is anæsthetised during an experiment and allowed to recover from the anæsthetic.

A licensee, through inadvertence, performed 54 inoculation experiments in excess of the number allowed by his certificate.

Another licensee, not understanding that joint experiments are reckoned to both of the licensees, took part in the performance of eight experiments in excess of the number allowed by his certificate.

By direction of the Secretary of State a suitable admonition was addressed to the licensee in each of the above cases.

In the month of April 1905 the attention of the Secretary of State was directed to certain experiments which were performed in 1903 and the early part of 1904 by persons not holding a licence under the Act 39 & 40 Vict. c. 77. The experiments consisted in vaccinating dogs against distemper and then exposing them to infection, the object being to test the efficacy of a method of vaccination as a safeguard against this disease. The Secretary of State thereupon caused inquiries to be made, and from these it appeared that the experiments, in some instances at least, had been accompanied by pain, and were, therefore, illegal. The persons, who were not aware that their experiments were of such a kind as to come within the provisions of the Act, were suitably admonished and warned against any similar illegal action in the future. The matter was not brought to the knowledge of the Secretary of State until it was too late for further proceedings to be taken if such had been considered necessary. It is as well to point out here that to expose an animal to an infectious and painful disease like distemper is a proceeding calculated to cause pain within the meaning of the Act, and that such experiments can only be legally performed by a person

holding a licence and appropriate certificates.—I have the honour to be, Sir, your obedient servant,

G. D. THANE, *Inspector.*

The Right Hon. HERBERT JOHN GLADSTONE,
Secretary of State for the Home Department.

IRELAND

8 ELY PLACE, DUBLIN,
April 26th, 1906.

SIR,—I beg to submit Tables showing the experiments performed in Ireland during the year 1905, under the Act 39 & 40 Vict. c. 77, together with a list of the Registered Places in Ireland.

Twelve licences were in force during the year; of these four expired, and two were renewed. One new license was granted.

The certificates in existence or allowed were :—

A.	to	4	licensees.
B.	„	7	„
C.	„	3	„
E.	„	2	„
EE.	„	3	„
F.	„	1	licensee.

One expired during the year, and six new ones were allowed.

The experiments performed number 218; 106 being under licence alone, and 112 under certificates. Ten licensees performed experiments. Twenty certificates were in force among 12 licensees, of whom 10 performed experiments, viz. :—

Under Certificate		A.	88
„	„	B.	14
„	„	C.	8
„	„	F.	2

The animals experimented on were :—

Guinea pigs	55
Birds	53
Rabbits	48
Cattle	27
Mice	14
Dogs	13
Cats	2
Horses	2
Goats	2
Sheep	2

The experiments were mainly pathological inoculations, done for the purposes of the investigation or diagnosis of various diseases, such as canine rabies, tuberculosis, cancer, glanders, and typhoid fever. A few were physiological, for the investigation of the functions of the thymus gland, and of the effects of chloroform and ether on renal activity. All of these seem to have been of a reasonable character and intended to serve useful purposes in the elucidation of the phenomena of disease or of vital functions. They are reported to have been free from pain.

Experiments numbering eight were performed in illustration of lectures, to demonstrate the phenomena of circulation and respiration and of nervous control. In these experiments, two dogs, two cats, and four rabbits were employed.

Some of the investigations were devoted to the study of diseases in cattle, horses, goats, and sheep, and seem to be useful and of economic value.

The registered places were inspected and their condition found satisfactory. The inspectors in Belfast and Cork report that in those places the provisions of the Act have been satisfactorily complied with.—I have, &c.,

W. THORNLEY STOKER,
Inspector for Ireland.

To the Right Honourable
The Chief Secretary to the
Lord Lieutenant of Ireland.

This Report gives a clear answer to certain false statements alleged against experiments on animals. It shows that more than 90 *per cent.* of these experiments are inoculations, with a few feeding experiments, administrations of substances by the mouth, or abstractions of a minute quantity of blood for examination. *In no instance has a certificate dispensing with the use of anæsthetics been allowed for an experiment involving a serious operation. In no case has a cutting operation more severe than a superficial venesection been allowed to be performed without anæsthetics.* It shows, also, that the results, in a very large number of these inoculations, are negative, painless, not even inconvenient.

The Report shows, also, that the vast majority of all experiments are inoculations made on the smaller animals; and that the larger animals (dog, cat, horse, mule, or ass) are seldom used for inoculation.

It shows, also, that a great proportion of these inoculations are made in the direct practical service of the public health and the public purse: to standardise drugs, to ensure the purity of food and of rivers, to protect flocks and herds, and to decide quarantine. Government Departments, County Councils, Municipal Corporations, and a Royal Commission made more than one-third of the total number of inoculations; and the Imperial Cancer Research Fund made more than one-third, mostly on mice; and a sixth was made over the testing and standardising of sera and of drugs.

The operations performed under the License + Certificate B, or B + EE, or B + F, were 3 per cent. of the whole number of experiments. The majority of the

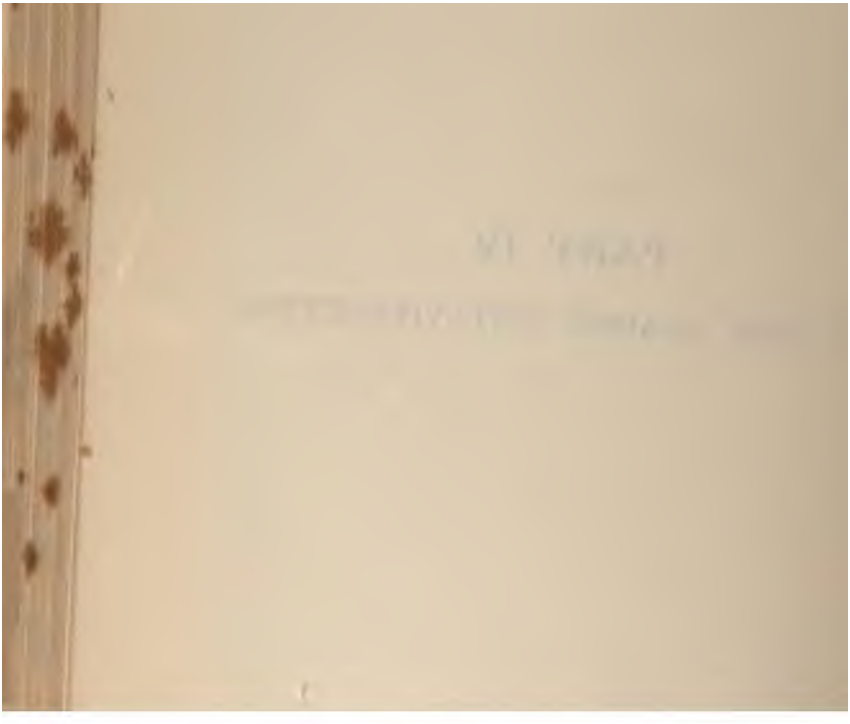
animals were neither cats nor dogs. They can hardly be compared to the same number of the larger animals mutilated by breeders and farmers: for these mutilations may be inflicted, and are inflicted, without an anæsthetic. They can hardly be compared to the same number of pheasants or rabbits wounded, but not killed, in sport; for the animals wounded in sport get no subsequent care, and, if they are in pain, nobody need put them out of it. They may fairly be compared to the same number of pet animals that have undergone surgical operations, under anæsthesia, at the hands of a skilled veterinary surgeon; only with this difference, that many of them lose health, or suffer disablement or disease, and so die or are killed; but, if the wound suppurates, the animal must be killed, and, after the wound has healed, the animals are not necessarily, or even generally, in pain. And there must be no *further* experiment without anæsthesia. *No observations or stimulations of a character to cause pain are allowed to be made without the animals being anæsthetised.* It is evident that good care is taken to ensure an irreducible minimum of pain.





PART IV

THE CASE AGAINST ANTI-VIVISECTION



THE CASE AGAINST ANTI-VIVISECTION

[The following pages are taken, with a few changes and omissions, from a pamphlet which I published in 1904. I am glad to say that the tone of the Anti-Vivisection Societies is not quite so bad as it was a few years ago; but I think that what I wrote in 1904 is still fairly accurate.]

I. Anti-Vivisection Societies

The early history of the anti-vivisection movement is given in a pamphlet by Dr. Leffingwell, of Brooklyn, entitled "The Rise of the Vivisection Controversy"; and in a pamphlet published by the National Anti-vivisection Society, entitled "Dates of the Principal Events connected with the Anti-vivisection Movement." Dr. Leffingwell calls attention to a fact not generally known—that the movement, in this country, was begun by the medical journals. The *Medical Times and Gazette* in 1858, the *Lancet* in 1860, and the *British Medical Journal* in 1861 condemned in a very outspoken way certain experiments made on the Continent, and raised the question whether these or any experiments on animals could be justified. Later, in 1872, the *Medical Times and Gazette* declared outright that all experiments, from the time of Magendie onward, had done nothing for humanity that could be compared to the discovery and use of cod-liver oil and bark. In 1874, the Royal Society for the Prevention of Cruelty to Animals took proceedings against those who had made certain ex-

periments at Norwich during a meeting of the British Medical Association. These experiments, and the publication of the *Handbook of the Physiological Laboratory*, roused public comment; and during 1875 the opposition to all experiments on animals took more definite form. On June 22nd, 1875, the Royal Commission was appointed; on January 8th, 1876, its report was dated; and on August 15th, 1876, the present Act received the Royal assent.

At the time when the Royal Commission was appointed, the only anti-vivisection society was that which Mr. Jesse had just started; and if any one will read Mr. Jesse's cross-examination, by Professor Huxley, before the Royal Commission, he will not attach much importance to that society. The National Anti-vivisection Society was founded in November 1875; the Irish Society, the London Society, and the International Association in 1876; the Church Anti-vivisection League in 1889, the Humanitarian League and the National Canine Defence League[1] in 1891, and the British Union about 1898. These dates show that the oldest of these societies came after the Royal Commission, not before it; the first societies and the Royal Commission were alike the expression of a widespread opinion, thirty years ago, that experiments on animals ought either to be forbidden or to be restricted. This same opinion had been favoured, fifteen years before that, by the representative journals of the medical profession. We have seen something of the work of the medical profession; let us now see something of the work of the societies.

[1] These two societies have other purposes beside that of opposition to experiments on animals.

The chief anti-vivisection societies in this country are the National Society, the London Society, the British Union, the Church League, and the Canine Defence League. In February 1898, the National Society declared itself in favour of restriction; it set before itself abolition as its ultimate policy, and restriction as its immediate practical policy. Thus, at the present time, these societies are divided into two parties: one asks for restriction, another asks for nothing short of abolition. This division between them, and the tone of the National Society toward the smaller Societies, waste their energy and their funds, and hinder them from working together. The National Society, in its official journal (January 1902), speaks as follows of this schism, in a leader entitled "The Folly of our Subdivisions":—

"Nobody seems to know how many Anti-vivisection Societies there are. A few hundred Anti-vivisectionists divide themselves up into divisions, subdivisions, coteries, and cliques, without order, without discipline, without cohesion. The Anti-vivisectionists between them all contribute but a few thousands a year, and dribble them around among multitudinous antagonistic associations. . . . The pitiful absurdity of the disunion fostered by some Anti-vivisectionists was illustrated very forcibly last year by the issue of a prospectus of a Society with a world-embracing title, in which its promoters declared that irreparable injury would be inflicted upon our cause if electoral work were not taken up by *them*. . . . The accounts of this stupendous organisation showed that its total expenditure for the year was £13, 19s. 4d., out of which ten shillings was devoted to 'electoral work.' . . . A much graver injury is done to the cause of mercy by the deplorable waste of money spent in perfectly unnecessary offices and salaries. We say that one office would amply suffice for all the work, and that one office

would not need half-a-dozen paid Secretaries. The existence of many quite needless Societies cannot be justified on any grounds of humanity combined with common sense."

Nothing need be added to these very grave admissions, written by Mr. Coleridge himself. He proposes a very simple remedy for these "quite needless" societies:—

"The National Society, as the chief Anti-vivisection organisation in the world, is always ready to put an end to this grievous waste by receiving into its corporation any of the smaller Societies."

But the leaders of smaller societies have two grounds of complaint against Mr. Coleridge's society: they do not believe in his policy, and they will not submit to his "discipline." They call his society "the weak-kneed brethren," and say that its policy is "miserable, cowardly, and misleading"; and they take it ill that he so often accuses them of inaccuracy. He refers again and again (see the official journal of the National Society) to this mode of discipline:—

December 1901.—"I decline to be made responsible for the 'anti-vivisection party.' There happen to be small anti-vivisection associations whose chief occupation is the dissemination of quite inaccurate pamphlets. I have nothing to do with them, and cannot prevent anything they choose to do."

January 1902.—"Time after time has this sacred cause been undermined and betrayed by its professing friends by their reckless habit of making erroneous statements."

March 1902.—"I am quite aware that with many of my opponents in the exclusive total-abolition coterie, the motives that actuate them are far removed from the question of the salvation of the wretched animals, and

have their foundation in emotions that seem to me singularly unworthy and petty."

May 1902.—"As representative of the National Society, I have again and again written to the representatives of some of the smaller anti-vivisection societies, protesting in plain terms against their publication of inaccurate statements."

No society could submit to be thus taken to task four times in six months. The Church League writes to him, "What the Church League may or may not think fit to say does not in the very least concern you, who are not a member of the League. Interference in such a matter from an outsider is an obvious impertinence." Such rejoinders are met, in their turn, by angry leaders, "A Stab in the Back," "Stabs in the Back," in the National Society's official journal; and the Hon. Secretary of the London Society, who is a lady, is accused of want of chivalry for Mr. Coleridge. The leader, "A Stab in the Back" (April 1902), is a curious instance of the tone of one anti-vivisection society toward another:—

"The time when a man is assailed by a large section of the press, threatened with violence by laymen, attacked on points relevant by vivisectors and points irrelevant by their supporters, is scarcely the moment that a generous rival would have chosen for hurling a dart; and yet, incredible as it may appear, the Honorary Secretary of another Anti-vivisection Society, seizing an opportunity afforded by an article in the *Globe*, enters the arena, and, by a letter repudiating any connection with Mr. Coleridge, appears to sanction the unfriendly criticisms expressed in that paper. It needed no chivalry to refrain from writing such a letter. A small amount of good taste would have amply sufficed. . . . This letter, which will convince the public of nothing but the writer's lack of

taste, might well be ignored were it not that it is but one of the many attacks made by members of other societies, either by open statement or innuendo, against the Honorary Secretary of the National Society."

But we cannot wonder at these occasional stabs. For the National Society does not stop at charging other societies with inaccuracy. It makes yet graver charges against them. Here are three made by Mr. Coleridge's society against Miss Cobbe's and Mr. Trist's societies :—

March 1901.—"The February number of the *Abolitionist* contains a leading article in which allusions are made to subjects that are never discussed by decent people even in private. As the leading organ of the Anti-vivisection movement, we enter our solemn protest against the publication of this unspeakable article, which must inevitably inflict the gravest injury upon our cause."

February 1903.—"It is our duty to inform our readers that Mr. Trist has published the correspondence, but that he has mutilated it, omitting some of his own letters altogether, and excising whole paragraphs of Mr. Stewart's letters."

June 1903.—"Our amiable contemporary, the *Abolitionist*, is good enough, in a long article in its last issue, to suggest to those preparing the libel action against Mr. Coleridge what are the most vulnerable points in his armour."

Thus divided in policy, and quarrelling among themselves, these societies are still agreed in appealing to the public for approval and for money. Here the London Society's opposition to the National Society comes out clearly. In its annual report (1903) the London Society says :—

"Join a really effective Society with a frank and straightforward policy—namely, the London Anti-vivisection Society, 13 Regent Street, London, S.W. This

is a National and International organisation. It has greater medical support than any other. It is the most 'alive' humane organisation in the world. . . . Get into touch with the society. Write to us. We shall be glad to hear from you and answer any questions."

"If you can provide for the Society's future in your Will, may we beg of you to do so? If you agree, pray do it now. Thousands of pounds have been lost to the Society and the Cause by the fatal procrastination of well-meaning friends. The pity of it! Legacies should be left in these *exact* words: 'To the *London* Anti-vivisection Society.' CAUTION. It is of great importance to describe very accurately the *Title of this Society*—namely, THE LONDON ANTI-VIVISECTION SOCIETY—otherwise the benevolent intentions of the Donor may be frustrated. PLEASE NOTE.—Those charitable persons who have left money to the Society would do well to notify the same to the Secretary."

Contrast the tone of this appeal for money with the tone of the Report:—

"Your Society are glad to note that the Christian Churches are becoming alarmed at the pretensions of scientific authority. . . . The Christian laity has been largely uninstructed or misinformed on this grave question. . . . Happily, the signs of the times are propitious; not all of the leaders of religious thought in this country have succumbed to the dictation and pretensions of the professors of vivisection . . . a base and blatant materialism, a practice which owes its inception to barbarism, and which has developed in materialism of the lowest possible order."

Surely such eloquence should avail to tear the money even out of the hands of the dying, lest the National Society should get it. The National Society, oddly enough, also says: "CAUTION.—It is of great importance to describe very accurately the *Title of this Society*

—namely, THE NATIONAL ANTI-VIVISECTION SOCIETY—otherwise the benevolent intentions of the Donor may be frustrated." I do not know which of these two societies is the inventor of this phrase. Still, it is not improbable that the National Society receives more money than all the smaller societies together. Of course, we cannot compare the working expenses of an anti-vivisection society with the working expenses of the Society for the Prevention of Cruelty to Animals, or the Society for the Prevention of Cruelty to Children. The former of these two societies in one year obtained 8798 convictions; in one month alone, 689 convictions; and it paid the full costs of committing 34 of the 689 to prison. The Society for the Prevention of Cruelty to Children has an equally good record. But an anti-vivisectionist society cannot show results of this kind. Nor can we compare its working expenses to those of a missionary society; for the missionaries give direct personal service to their fellow-men. But we can fairly compare an anti-vivisection society to an anti-vaccination society or a Church of Christian Science. That is to say, it is a publishing body. In 1902, the National Society's expenditure, in round numbers, was £970 on printing and stationery; £1193 on rent, salaries, and wages; £1255 on books, newspapers, periodicals, &c., including the *Illustrated Catalogue* and the *Hospital Guide*; £1380 on lectures, meetings, organising new branches, &c.; and about £500 on all other expenses. Let us take, to illustrate these figures, what the National Society says from time to time in its official journal:—

June 1899.—(From the Society's Annual Report): "The whole controversy has been collected and published in pamphlet form by your Society, and more than 10,000 copies have already been issued to the public.

Over 200 people have joined your ranks and become members of the Society in consequence of it, while two cheques of £1000 each were received by Mr. Coleridge in aid of the cause."

June 1899.—"We have received more money within the past six months than we got in any two years previously."

June 1899.—"We cannot better employ the funds at our disposal than in securing the constant help of experts to insure the accuracy of all our statements, and in sending well-informed lecturers to every city in the kingdom."

June 1900.—(From the Society's Annual Report): "The receipts of the society from subscriptions and donations show an increase over those of the previous year. This increase in itself, however, would hardly have justified the increase in the expenses which it has been found necessary to incur in almost every department, and especially in the distribution of pamphlets and papers, had it not been for some legacies which fell due, notably one from ——, of £6386."

May 1901.—"With heartfelt gratitude we have once more to announce that the National Society has received a gift of a thousand pounds from an anonymous donor. Nothing could be more opportune for the Cause than this munificent support, coming as it does just as the issue of 20,000 copies of Mr. Stephen Coleridge's *Hospital Guide* has been made at so great a cost to the Society."

June 1901.—"Our editorial table is buried deep in press cuttings from all parts of the kingdom."

March 1902.—"We employ two press-cutting agencies to send us cuttings from the journals of the whole English-speaking world."

July 1903.—"We start branches in various towns, and send lecturers to speak at working men's clubs and debating societies. All this means a very large expense. We very often issue a pamphlet likely to do good by the tens of thousands. Last year we issued 50,000 copies of the 'Illustrated German Catalogue of Vivisectional Instruments and Appliances.'"

The smaller societies, of course, spend their funds in the same sort of way. Thus the National Canine Defence League says that its anti-vivisection work, the most important of all its works, is earnestly carried forward by (1) The Writer's League, in a ceaseless flow of letters to the press; (2) The circulation of lists of hospitals free from the shameful practice; (3) The publication of twenty-one strong leaflets on the subject; (4) The circulation of 300 copies of a book on the subject. This society in two years sent out 650,000 leaflets and pamphlets; but they were not all of them about experiments on animals. Another Society, in a report published in 1902, enumerates the methods which it employs for "the education of the public at large." These include (*a*) the publication of literature; (*b*) the holding of public meetings in all parts of the United Kingdom; (*c*) the delivery of lectures with or without limelight illustrations; (*d*) participation in debates even with high scientific authorities; (*e*) inducing the clergy and ministers of all Churches to deliver sermons dealing with the subject; (*f*) organisation of a press bureau, through which the newspaper press of the country is watched, and correspondence and articles contributed. This Society has also a van, "the only one of its kind in existence. No sooner is our winter and spring campaign concluded than the van takes up the thread of the work and carries it on through the summer, and it may truly be said that the track of the van across country is white with the literature which the van circulates on its educational mission."

It is evident, from these and the like statements, that these Societies, during the last quarter of a century, have published a vast quantity of literature. We must

examine the style of that literature during some recent years, and the arguments which it puts forward. But, before we do this, let us consider what attitude is taken by these Societies, or by well-known members of this or that Society, toward certain problems and interests that closely concern them.

I

They do not hesitate to take advantage of all those improvements of medicine and surgery which have been made by the help of experiments on animals. They denounce the work of the present; but they enjoy all the results of the past, and will enjoy all those of the near future. "If anything of value to medicine has been discovered by vivisection, it would be as absurd to reject it on that account as it would be to abandon Ireland because centuries ago we took it by force." And again: "We are no more morally bound to reject benefits acquired by indefensible means than are the descendants of slaveholders bound to abandon wealth originally acquired by the detestable abomination of slavery." And again, the *Animal's Friend* (November 1903) takes as further instances the benefits derived from body-snatching, political assassination, and the French Revolution. But, in the matter of experiments on animals, it is the very same men and women who denounce these experiments and who profit by them. What should we say of an anti-slavery reformer who was himself drawing a vast income out of the slave trade?

But there is one gentleman, and, so far as I know, only one, who did carry his opinions into practice. He told the story at a debating meeting—how his little girl

had a sore throat, and the doctor wanted to give antitoxin, and he forbade it, and the child recovered. "Of course," he says, "it was only an ordinary sore throat." Truly, a great victory, and a brave deed, to make an experiment on your own sick child.

II

The attitude of these Societies toward sport may seem at first sight purely negative; but it is worth study. I have the honour of knowing a very eminent physiologist who will never shoot, because he thinks it cruel—a man much abused by the National Society. And Lord Llangattock, the President of that Society, is well known as an "ardent sportsman."

This contrast is of some interest. Let us see what the National Society says about sport. Of course, it is not bound to attack sport. But the reasons which it gives for remaining neutral are to be noted.

1. It says, very truly, that it is in great part supported by sportsmen.

2. It says, further, that the cruelties of sport lie outside its own proper work:—

"Our opponents frequently ask us why we do not attack some form of cruelty other than vivisection, which they consider more heinous. Our Honorary Secretary recently summarised this argument in his own amusing manner thus: We must not arrest the man in Tooting for kicking his wife till we have stopped the woman in Balham starving her children, and we must not arrest the woman in Balham for starving her children until we have stopped the man in Tooting kicking his wife." (1901.)

Later (1903) the *dramatis personæ* are a man in East Islington jumping on his wife, and a woman in West

Islington stabbing her husband. But this argument, of course, will not hold. For it is the same men who denounce wounds made (under anæsthetics) for physiology, and who make wounds (without anæsthetics) in sport.

3. It says that the "object" of the sportsman is to kill; but the "object" of the experimenter is to torture:—

"There is a vast difference between the killing of animals and the torturing of them before killing them. The object of the sportsman is to kill his quarry; the object of the vivisector is to keep his victim alive while he dissects it."—Mr. Wood (1903).

"The object of the sportsman is to kill, and the object of the vivisector is to keep his victim alive while he cuts it up."—Lord Llangattock (1901).

"The vivisector is nothing if not a tormentor; the sportsman is not a true sportsman if he seeks to inflict pain on his quarry. . . . One (the pain of a horse falling on asphalt) is the result of an accident to be deplored, the other (the pain from an experiment) is done of devilish malice prepense."—Leader in the Society's official journal, (1899).

"I am not so mentally and ethically confused as to be unable to distinguish between the entirely different moral acts of killing and torturing."—Mr. Coleridge (1901).

Here are four statements. One is by Mr. Wood, the Society's lecturer; one by Lord Llangattock, its President; one is published in its official journal; and one is by Mr. Coleridge, its honorary secretary and treasurer. That is the sort of thing which seems good enough to the National Society to say to its friends in Parliament; this childish nonsense about the true sportsman and his quarry.

III

The attitude of these Societies toward the medical profession, and toward the Hospitals, must be studied. Let us look through some numbers of the official journal of the National Society, and see the attitude that it sometimes takes toward the medical profession :—

June 1899.—"The charm of this sort of thing is that you are always sure of the *post-mortem* if of nothing else."

July 1899.—"There is a disease, well known to the vestrymen of London, called 'the half-crown diphtheria.' This is common sore throat, notified as diphtheria because the vestry pays a fee of half-a-crown to the medical notifier."

December 1899.—"The patient died, made miserable by the effect of inoculations which even on bacteriological grounds gave no promise of success, but the scientific physician, nowadays, must inject something in the way of a serum."

March 1901.—"There will always be those who, unable to think for themselves or exercise their independence on therapeutic methods, are prone to bow down before authority which is self-assertive enough to compel the obedience of weak minds. Such men would inject antitoxin though every case died. They administer it not knowing why."

April 1901.—(From "Our Cause in the Press"): "What effort does the medical profession make to make clear to its clients what is well known to itself, that disease is the result of wrong living? Practically none at all. The medical profession as a whole have winked at sin, and have merely sought to antidote its results."

September 1901.—"Some day we shall have our

surgeons disembowelling us just to see what daylight and fresh air will do for the stomach-ache."

December 1901.—"The new medicine demands a mere laboratory habit; the patient is nothing, the disease everything. He is a test-tube; such and such reagents are needed to produce a certain result, and there you are. The patient's malady, be it what it may, is due to a microbe, a toxin, or a ptomaine; he must be inoculated with the serum or antitoxin which counteracts his disease, and this must be done not *secundum artem* but *secundum scientiam*, and the science means the inoculating syringe and so many cubic centimetres of filth wherewith to poison the man's blood and so cure his disease, though the victims die."

December 1903.—(From "Our Cause in the Press"): "Not only did we see great callousness in the field hospitals in South Africa, but conversation with the class that finds its way into our hospitals in England will reveal that a great deal of refined cruelty is constantly occurring."

Why does the official journal of Mr. Coleridge's society publish these things? For this reason—that it must attack those methods that were discovered by the help of experiments on animals. The medical profession uses these methods. Therefore, that profession must be attacked.

The same reason, of course, helps to explain the National Society's attack on the great Hospitals of London. It would take too long to tell here the whole story of that attack. Three charges were made against the Hospitals: (1) that they maltreat patients; (2) that they promote the torture of animals; (3) that they endow this torture at the cost of the patients. They were accused, to put it plainly, of treachery and fraud; and of course the Council of the King's Hospital Fund got its share of abuse. Mr. Coleridge said on this subject:—

1. (Annual meeting at St. James's Hall, May 1901): "How have Lord Lister, the vivisector, and his Committee distributed the Prince of Wales's Hospital Fund? They have so distributed this fund as to make it clear to hospital managers that the more they connect their hospitals with the torture of animals the larger will be the grant they may expect to get from the Prince of Wales's Fund. That fund, therefore, has been used as an insidious but powerful incentive to vivisection."

2. (Annual meeting at St. James's Hall, 1902): "Sheltering itself now in its most repulsive form behind those ancient and glorious institutions, founded and sustained for their Christ-like work of healing the sick, sapping their foundations and smirching their fair fame, malignant cruelty has taken up its position in its last ditch. There it has summoned to its aid vast interests, ancient prejudices, enormous endowments, and under illustrious patronage it has pilfered the funds subscribed for the poor."

With these statements before us (and it would be easy to add to them) we cannot doubt that the plan of campaign against all experiments on animals is also hostile to the Hospitals, whenever that hostility seems likely to be of the very least use to the cause.

Surely there are charities more worthy of subscriptions, donations, and legacies than these Anti-vivisection Societies. They quarrel among themselves; they spend vast sums of money on offices, salaries, press-cuttings, reprints, lectures and meetings, tons of pamphlets and leaflets. Their members denounce all experiments done now, while they enjoy the profit of all experiments done before now; they say that the object of the physiologist is to torture his victim out of

devilish malice prepense ; they accuse doctors of fraud, and lying, and refined cruelty, and madness, and winking at sin ; they blacklist and boycott the best Hospitals. And the whole costly business, these thirty years, has done nothing to stop these experiments ; they have increased rapidly. Surely, if a man wishes to help and comfort animals, he had better give his money to the Home for Lost Dogs, or the Home of Rest for Horses.

II. Literature.

We have now to examine the style of the literature of these societies. But, out of such a vast store of journals, pamphlets, and leaflets, we can only take one here or there.

From time to time a book or a pamphlet is, for good reasons, withdrawn. Thus, in 1902, the London Society withdrew *Dark Deeds*. (*The Shambles of Science*, now impounded, was published by a chairman of committee of the National Society, but not by that society.) In 1900 the National Society withdrew one or more pamphlets involving acceptance of Dr. Bowie's mistranslation of Harvey. In 1902 it withdrew and destroyed a whole store of diverse pamphlets, and appealed to its supporters to "refrain from circulating any literature not issued from our office by the present committee" ; that is to say, it warned them to distribute no literature but its own, and not all even of that. But the withdrawal of a few books and pamphlets makes very little difference ; and most of them are "revised" and brought out again. Take, for example, the *Nine Circles*. It was planned and compiled for Miss Cobbe ; Mr. Berdoe

was "urgently requested by her to point out to her any scientific errors or possible inadvertent misrepresentations of fact, and correct or expunge them"; and he "carefully read through the proof-sheets." The book purported to be an exact account, from original sources, of certain experiments, some made abroad, some in this country. It was attacked by Sir Victor Horsley at the Church Congress at Folkestone, October 1892, and was withdrawn, revised, and brought out again. Our only concern here is to see what the official journal of the National Society said of the revised issue. This official journal, the *Zoophilist and Animal's Defender*, was started in May 1881, under the shorter title of the *Zoophilist*. It speaks of itself as a "scientific journal," and as "the recognised organ of the anti-vivisection movement in England." It is published monthly, and may be obtained through any bookseller. In 1883 it was edited by Miss Cobbe; in 1884 by Mr. Benjamin Bryan; in 1898 by Mr. Berdoe. In 1903, Mr. Coleridge, apologising for an error made in it in 1898, says: "At that time I had not the control over its pages that is at present accorded to me." Thus it is, I believe, still edited by Mr. Berdoe, and is, or was in 1903, controlled by Mr. Coleridge. And we are bound to note here that Mr. Berdoe was in great part responsible for the *Nine Circles;* and in 1897 was responsible for certain statements as to the use of curare, which the Home Secretary, in the House of Commons, called "absolutely baseless."

Let us now examine the style of this "official journal." And, to begin with, what does it say about the *Nine Circles?* To make this point clear, let us put in parallel columns what was said by Sir Victor Horsley

of the original edition in 1892, and what was said by the *Zoophilist* in 1899 of the revised edition :—

Sir Victor Horsley, Oct. 1892.

I have taken the trouble to collect all the experiments in which cutting operations are described as having been performed by English scientists, and in which I knew anæsthetics to have been employed. These experiments are 26 in number. In all of them chloroform, ether, or other anæsthetic agent was employed. But of these 26 cases, Miss Cobbe does not mention this fact at all in 20, and only states it without qualification in two out of the remaining six. When we inquire into these 20 omissions in the 26 cases, we find in the original that again and again Miss Cobbe has, in making her extracts, had directly under her eyes the words "chloroform," "ether," "etherised," "chloroformed," "anæsthetised," "during every experiment the animal has been deeply under the influence of an anæsthetic," and so forth.

The "Zoophilist," July 1899.

A revised edition has been issued, which is a stronger indictment against the vivisectors than the original work. There were some half-dozen omissions in the first edition concerning the administration of anæsthetics in the preliminary operations, but the cruelty of the experiments was in no case modified by the fact that a whiff of chloroform was possibly administered, as stated in the reports, at the beginning of the operation. Our opponents may boast of their success in detecting the omission to dot the i's and cross the t's in the first edition of the *Nine Circles*, but there are some victories which are worse than a defeat. We have replaced the lantern with which we examined the dark deeds of the laboratories by the electric searchlight. The "researcher" will find it hard to discover a retreat where its rays will not follow and expose him.

For another instance of the inaccuracy of the *Zoophilist* we have what it said about Professor Sanarelli's experiments in South America on five human beings. Nobody defends him here. But the point is that the *Zoophilist* in 1899 said that they had all been killed ; and in 1902 admitted that they had all recovered. Or, for another instance, we have what it said in 1902 about the case of His Majesty the King. (For these statements, see *Zoophilist*, August 1902 and September

1903; also its report, October 1902, of Mr. Wood's speech at Exeter.)

But let us take a wider view. A journal, like a man, is known by the company that it keeps. Whose company does the *Zoophilist* keep? Why does it talk of *Our excellent cotemporary, Humanity—Our valiant cotemporary, Le Médecine—Our excellent cotemporary, The Herald of the Golden Age?* Again, among the journals that it quotes, some of them very frequently, are the *Topical Times*, *Broad Views*, *Modern Society*, *Madame*, the *Humanitarian*, the *Pioneer*, the *Vegetarian*, the *Voice of India*, the *Herald of Health*, the *Rock*, the *New Age*, the *Journal of Zoophily*, the *Homœopathic World*, *Medical Liberty*, and the *Honolulu Humane Educator*. This may be very good company, but it is not all of it the best company for a "scientific journal." Still, it may be better company than the American *Medical Brief*, the *Journal de Médecine de Paris*, and the Belgian *Le Médecine*. These journals, being veritable "medical journals," are quoted in the *Zoophilist* with the most amazing frequency and at great length; which is a compliment that they do not receive from other medical journals. They are, indeed, as vehemently anti-Pasteur and anti-antitoxin as the *Zoophilist* itself. Take what the *Medical Brief* says:—

"Bacteriology originated in Continental Europe, where the minds of a superstitious race were further unbalanced by constant delving in pathology, putrefaction, and morbid anatomy. When it spread to the new world, it also became blinded with the revolutionary and fanatical tendencies lying near the surface in such a civilisation."

"They say if you give a calf rope enough, he will hang himself. Bacteriology is equally clumsy and stupid. . . . What excuse can be found for the cowardice and fero-

cious ignorance which, under the shadow of the stars and stripes, resurrects the sentiment of the Middle Ages to protect the fraud, seeks to rob the individual physician of free judgment, and denounces him for failing to use the nasty stuff?"

"All Continental Europe is suffering from a sort of leprosy of decadence, mental and moral. The spiritual darkness of the people affects all the learned professions, but more especially medicine."

Such is the *Medical Brief*, which the official journal of Mr. Coleridge's society quotes incessantly, calling it "an American monthly of great ability and without a trace of the scientific bigotry and narrow-mindedness which is so prominent a feature in some of our own organs of medical opinion." Next we come to the *Journal de Médecine de Paris*. This is anti-Pasteur; the editor, Dr. Lutaud, came to London in 1899, and gave a lecture on "the Pasteur superstition" at St. Martin's Town Hall. From a report of it in the *Star* we may take the following sentences:—

"The result of the serum craze had been that the hospital was neglected for the laboratory. Microbes of all the diseases were found in perfectly healthy subjects. Microbes existed, but as a consequence, not a cause. Toxins which the seropaths professed to find were only the results of normal fermentation. The English public had always supported him in his fifteen years' struggle against Pasteurism."

Dr. Lutaud, says the National Society, is "the great authority." The *New England Anti-vivisection Monthly* in 1900 calls him one of "the brightest scientists of modern times." His *Journal de Médecine de Paris* recalls the *Medical Brief*:—

"To wish to apply the same methods of treatment, whether preventive or curative, for two morbid condi-

tions (a *wound* with the point of entry *abnormal* and an infectious *malady*) in essence so different, is to commit a gross error. . . . The sick are destroyed by that which cures their wounds."

These two "medical journals," the *Medical Brief* and the *Journal de Médecine de Paris*, are upheld by the National Society as though they were expert witnesses of irresistible authority, and are quoted with a sort of ceaseless worship in that Society's official journal. Also it quotes the *Herald of Health*; and *Medical Liberty*, "a monthly publication issued by the Colorado Medical Liberty League, Denver, Colo., whose eloquent editor seems to be an uncompromising foe to medical bigotry and monopoly, and humbugs of every description."

Such are the medical journals which support the *Zoophilist* as a scientific journal. Now let us take another point of view. Let us consider whom the *Zoophilist* praises, and whom it condemns. That, surely, is a fair test of an official journal. And we get a clear result. The late Lord Salisbury and Mr. Arthur Balfour are "notoriously pro-vivisectionist"; Lord Lister has "apostatised from the anti-septic faith"; M. Pasteur is a "remorseless torturer"; the late Mr. Lecky was "degenerate," because he "performed the *volte-face* and went over to our opponents"; and the late Professor Virchow was subjected to "scathing criticism" by one Paffrath, and was proved to be absurd. But its praises are given to a very different set of men.

There is no room here to note the lighter moods of the *Zoophilist*; its jokes about cats and catacombs, and two-legged donkeys and four-legged donkeys, and how to catch mosquitoes by putting salt on their tails—and it will even *break its jest on the dead*—but it rebukes another journal for levity, saying, *We regret to see our*

painful subject treated in this manner. No room, either, for its description of anti-vivisectionist plays, poems, novels, and sermons. Let us, to finish with, take a few statements from its pages, almost at random ; some of them are reprinted there from other sources. The supply is endless; let us limit ourselves to six of them :—

1. "As other bacteria (beside those of malaria) were found not to bear sunlight or air, but to habitats in *loca scuta situ* (? to inhabit *loca senta situ*), in filth and noisomeness, their habits and customs preached again the old doctrine, 'Let in sun and air and be clean,' as earnestly as those who thought health was due to sun and air and water and fire, the four old elements, and act accordingly, without dissecting hecatombs of animals to prove a thousand times over that if you boiled or baked or drowned or freezed living creatures they would die, or that microscopic parasites did pretty much what visible parasites have been always known to do." (Loud applause)—Report of a speech by the Bishop of Southwell (1901).

2. "It is just as well that you should have heard what the clever level-headed lawyer (Mr. Coleridge) thinks about this abominable conspiracy of cruelty and fraud and impious inquisitiveness which is called vivisection. (Cheers.) . . . We are sending out on the world in every direction multitudes of young men who have been trained as surgeons, and they have lived by cutting (reference here to the medical students in *Pickwick*), and we are sending these young men out with this *cacoëthes secandi*, this mania for cutting for the mere sake of cutting. I should not be surprised if they tackle our noses or our ears, and set about mutilating us in that way."—Archdeacon Wilberforce (1901).

3. "The task of the crusader against vivisection is not to reason with the so-called scientist, not to truckle to pedants in the schools, or palter with callous doctrinaires,

but to inform and arouse the people; and when John Bull is prodded from his apathy, and startled from his stertorous snore, he will rise and bellow out a veto on the elegant butcheries of pedantic libertines, and rush full tilt with both his horns against their abattoirs of cruelty and passion, pharisaically vaunted as research, until the gates of hell shall not prevail against him."—The Rev. Arthur Mursell (1901).[1]

4. "It has been my experience of anti-vivisection among Romanists, that nothing suited my purpose better than taking it for granted that the worshippers of St. Francis, St. Bernard, &c., must, *of course*, be on our side."—(1902.)

5. "Given money, and influential patronage, the vivisector now expects a time after his own heart, while professedly engaged in investigating the supposed causes of cancer, or the transmissibility of tuberculosis. He can inflict the most horrible and prolonged tortures on miserable animals, with such a plausible excuse in reserve, that he is endeavouring all the while to find cures for the ailments of high personages and millionaires."—(1902.)

6. "The day of drugging and scientific butchery is drawing to a close. Already the calm, reassuring voice of the new Life Science, loud and clear to the few, is faintly audible to the many. The sharp, crucial knife, with its dangerous quiver so dear to the heart of the surgeon, the poisonous drug, will be things of the past. Wisdom, thy paths are harmony and joy and peace."—(1902.)

Such is the frequent level of the *Zoophilist*, the official journal of the National Society, edited by Mr. Berdoe, controlled by Mr. Coleridge. Let us now take one

[1] Even the *Zoophilist*, which quotes this speech from the *Clapham Observer*, seems to feel that it might have been put more simply.

more of that society's publications, a pamphlet entitled *Medical Opinions on Vivisection*. Here, if anywhere, should be the society's stronghold. If it could show a large and important minority of the medical profession opposed to all experiments on animals, its power would be greatly increased. On three occasions, many years ago, the medical profession did express its opinion. At two of the annual meetings of the British Medical Association, and at a meeting of the London International Medical Congress, resolutions were passed affirming the value and the necessity of these experiments. At one of these meetings there was one dissentient vote; at one, two;[1] at one, none. These three meetings were truly representative; they were the great meetings of the clans of the profession, from all parts of the kingdom, for a week of practical work tempered by festivities. What more could any profession do than to go out of its way three times that it might record, in fullest assembly, its belief? And most certainly it would do the same thing again, if it thought that any further declaration were needed.

There are in this country about 40,000 medical men. The National Society's pamphlet quotes 39, or one in 1000. It could quote more; but we must take what it gives us. Of these 39, we may fairly exclude Professor Koch, Sir Frederick Treves, and the late Sir Andrew Clark, who would certainly wish to be thus excluded. Sir Frederick Treves, who is quoted with a sort of explanatory note, has told us in the *Times* what he thinks of the way in which his name has been used; Sir Andrew Clark is quoted, also with an explanatory note, for an *obiter dictum;* and Professor Koch for no discoverable reason. That leaves 36. Of

[1] I think it was two; it was either one or two.

these 36, at least 11 (probably more) are dead ; one died about 1838, another was born in the eighteenth century, another died more than twenty years ago. Of the remaining 25, one is Dr. Lutaud, one is Mr. Berdoe, one an American doctor, not famous over here, one a veterinary surgeon, one (I think) opposed to vaccination, and three inclined to homœopathy ; one has mistranslated Harvey to the advantage of the National Society's cause, one has written *Hints to Mothers*, and one has written *How to Keep Well.* Of these 25 gentlemen, one belongs to a homœopathic hospital, two to provincial hospitals, and one to a hydropathic institute and a children's sanatorium ; the rest of them hold no hospital or school appointment of any sort or kind. I may be wrong over one or two of these names ; but, so far as I can see, I have given an exact account of the value of these *Medical Opinions on Vivisection.* And, if we take the dates of these opinions, we find one in 1830, one in 1858, and seven in 1870–1880. Anyhow, what is the value of an opinion that all experiments on animals are *arrant and horrible Sepoyism wearing the mask of Art and Science?*

Let us leave the National Society, and turn to the Canine Defence League, and examine that part of its literature which is concerned with experiments on animals. Take the following sentences from pamphlets 179 and 204 :—

"Among the general public the majority are under the impression that these so-called physiological experiments are conducted under the influence of anæsthetics, and that the subjects are rendered insensible to pain ; this, however, is *not the case*, and I am informed that a large proportion—considerably more than half—of the licenses

dispense with anæsthetics entirely. The phenomena of pain are absolutely essential to any practical issue."

"All diseases have a mental or spiritual origin. Upon this subject a large treatise might be written. I have carefully thought this matter over, and can come to no other conclusion. Can we imagine any wild bird confined to its nest with rheumatism, or neuralgia, or consumption, or asthma, or any other affection whatever? I believe them all to be entirely free from disease; that is, all which have retained their freedom, and thus have not come under the baneful influence of man. Take, again, the fishes, and ask whether any fisherman ever caught a fish found to be diseased. This subject is an interesting, though a somewhat melancholy one."

Next, as an example of the literature of the London Society, let us take a speech made at St. James's Hall, May 26, 1903, by Dr. Hadwen, of Gloucester, who is also vehemently opposed to vaccination. He and Lieutenant-General Phelps, at the time of the disastrous smallpox epidemic in Gloucester in 1896, were leaders of the anti-vaccinationists. It would be easy to give other instances of the sympathy between anti-vivisection and anti-vaccination. But our business is not with Dr. Hadwen at Gloucester, but with him at St. James's Hall. He says to the London Society:—

"We are told we must pay attention to what the experts tell us. My opinion is this: If there is one person in the whole of God's creation that wants looking after, it is the expert. (Laughter.)"

Of the House of Commons, he says:—

"If there is one thing in the world that will move a member of Parliament, it is to know that any particular policy will carry votes along with it. (Hear, hear.) You can bring any member of Parliament to your knees as

long as you show him that he has his constituency at his back; and with all due respect to our noble chairman, I am bound to say that my experience of members of Parliament is this—that their consciences go as far as votes, and do not extend very much farther." (Laughter and applause.)

He describes an imaginary experiment under curare, and is interrupted by a cry of "Demons!" He goes on :—

"Yes, madam, they are demons. (Applause.) I know no other word to describe experimenters who can submit sentient and sensitive creatures, almost human in intelligence and faith, to diabolical experiments, whilst their victims are rendered helpless and voiceless by a hellish drug. (Applause.) I cannot understand how in a land like this, that boasts of her Christianity and of her liberty, men, women, clergy, and politicians can allow this cowardly science to stand before us, and this demoniacal work to be carried on. (Loud cheers.)"

We have now seen something of the style of the literature of these Societies ; and, in the next chapter, we will consider its arguments. I do not deny that its style is sometimes at a higher level than the examples which I have quoted. But I do say that I could fill a book of 100 pages with quotations from journals or pamphlets of the last few years, all of them on the lower level. And in this chapter I have practically quoted nobody but those who are the leaders of the opposition to all experiments on animals. The official journal of this Society, the annual report of that Society, the leaflets which are sent in answer to a formal request for literature—I have quoted these, as they came to hand, just going through them and marking those passages which were to my purpose.

III. Arguments

We have seen that the societies arose out of the Act, and not the Act out of them; that they are divided or hostile; and that they have next to nothing to show for all the vast sums which they have received. Also we have noted the style of literature which they send broadcast over the country; and the "medical journals" and "medical opinions" that are in favour of the cause; and the general tone and frequent level of the official journal of the National Society. Still, a good cause may be ill served; nobody minds, after all, the style of a thing, so long as it is true. Let us come to the heart of the matter. What is the nature of the arguments and evidences of these societies? They desire to bring about the absolute prohibition, as a criminal offence, of all experiments on animals. By what facts, what records, what statistics, do they maintain this attempt to mend or end the present Act?

Here, at the risk of repetition, let me make quite clear what they are fighting against. Nine out of ten experiments are bacteriological. That is to say, 90 or 95 per cent. Of these inoculations, more than a third are made in the direct service of the national health, and as it were by the direct orders of Government. A vast number of them are wholly painless; nothing happens; the result is negative; the thing does not take. Some are followed by disease, and the animal is painlessly killed at the first manifestation of the disease, or recovers, or dies of the disease. The fate of that animal is the fate of all of us; it has got to die of something, and it dies of it. Anyhow, the talk about torture-troughs and cutting-up has no place here; and the word vivisection,

by a gross and palpable abuse, is false nine times out of every ten. Of the remaining 10 per cent. of all experiments; in those that are made under the License alone, or under the License *plus* Certificate C, the question of pain does not arise. The animal is anæsthetised, and is killed under that anæsthetic. The remaining 3 per cent. of all experiments are those that are made under the License *plus* Certificate B (or B + EE, or B + F). The initial operation is done under the anæsthetic; the animal is allowed to recover; it may be, practically, none the worse for it. Or it may be the worse for it, and therefore die, or be killed. But Certificate B is *not* allowed for any infliction of pain on the animal through the operation wound, and never will be.

Here are two sets of experiments: those under Certificate A, and those under Certificate B. One is 90 per cent. of all experiments; the other is 3 per cent. Nine out of ten experiments are inoculations, and the operation of the tenth is done under an anæsthetic. That is the first fact, which we must fix in our minds, before we consider the arguments of the societies.

Next, the dates and the sources of their evidence. They wish to stop the experiments that are now made in this country. They are bound, therefore, to produce "up-to-date" evidence, and from home sources; not that which is thirty years old, or comes from sources far away. This present use of animals, here and now, under the restrictions of the Act, is what they are fighting; they are bound to draw their instances from here and now.

But this would not suit them at all: they could not bear to be thus limited to here and now. Their arguments and their instances extend over thirty or more

years, and are drawn from all parts of the world, from the United Kingdom, the United States, France, Germany, Italy, from every country. Journals of Physiology, text-books, reports, medical journals, British and foreign, are ransacked to find evidence for the cause; there is a regular system, year in year out, a sort of secret service or detective force, a persistent hunting-up of all scraps and shreds of evidence. One society advertised, in a daily paper, that it wanted confidential communications, from medical students, as to the practices of the laboratory. Another, seeing the chance of a prosecution, says, "Special inquiries were made on the subject, and the society's solicitor went to Belfast to conduct these inquiries on the spot." All this espionage is sure now and again, in thirty years, to detect something which it can magnify into a scandal. And when a fault is found, even a little one, oh the joy in the ranks of the societies. And, at once, the fault, exaggerated, and highly coloured, is made a *locus classicus*, a commonplace of every drawing-room meeting. What is the date of it, what was the place of it? Was it long ago, was it far from here? Still, never let it drop; what one did then, they are all doing now, all of them of malice prepense; let us proclaim the blessed news from every platform; and please remember us in your Wills.

Among the arguments against all experiments on animals, is this very common argument, that the truth about them is too horrible to be told. "We dare not produce our brief," says the Rev. Nevison Loraine, at the annual meeting of the London Society in 1901; "it is only the courage of a lady that dares to produce tales so harrowing as those that have been briefly alluded to to-day; and it is part of the weakness of

our cause with the public that we cannot tell the whole story." But, not long ago, the courage of two ladies, officers of a Swedish Anti-vivisection Society, honorary members of Mr. Coleridge's society, did produce a book full of harrowing tales; they told the whole story to the Lord Chief Justice and a jury. Was not that producing their brief? *I have here in my pocket something I have not got the nerve to read to you*, says Archdeacon Wilberforce, at the annual meeting of the National Society in 1901; and the next minute a lady in the audience is crying out, *Do not go on, we cannot bear it;* and he says, *You have got to bear it. Good God, they have got to suffer it.* Is not that producing his brief? Mr. Coleridge, in 1902, sends out 12,000 copies, just to begin with, of an illustrated German catalogue of laboratory instruments: *The question of thus scattering abroad this fearful document has been the subject of very grave consideration. . . . We have launched upon the world this terrible proof of what vivisection really is, with a full sense of our responsibility.* Is not that producing his brief? These things in the pocket, and fearful documents, and briefs that Mr. Loraine dares not produce, are apt to say little or nothing about anæsthetics, and to be silent over the fact that nine out of every ten experiments are bacteriological, and to over-emphasise experiments made many years ago or a thousand miles away. You bring the speaker down to now and here, to the text of the Act, to the reports to Government, to the Home Secretary's own words in Parliament; and you are told that they are all in a conspiracy, all liars more or less, and that the truth is in the societies, especially in one of them. Or you bring him down to the good

that these experiments have done, the lives that they have saved; and at once he is off like the wind:—

"The society does not concern itself with the results of vivisection, whether good or bad, and thinks it is beside the mark to discuss them." (Report of the Canine Defence League, 1903.)

"When the angel of pity is driven from the heart; when the fountain of tears is dry, the soul becomes a serpent crawling in the dust of the desert." (Colonel Ingersoll.)

"I make no pretence to criticise vivisectional experiments on the ground of their technical failure or success. I dogmatically postulate humaneness as a condition of worthy personal character." (Mr. Bernard Shaw.)

"The vivisector, when he stands over his animal, whether with anæsthetics or without anæsthetics, is creating, even if the physical health of the nation is enhanced by it, a moral shroud not only for himself, but a moral shroud the edges of which are continually extending into the thought atmosphere, and so deadening the national conscience at large." (Mr. Herbert Burrows.)

"The developed taste for blood and cruelty must in the end find its full satisfaction in the vivisection of human beings when they have the misfortune to come under the power of our future doctors." (Bishop Bagshawe.)

Here, in these five sentences taken merely out of the heap, is the ethical argument; so facile, so pleasant to self, so confident of a good hearing. No wonder that the societies, now that the facts of science are too strong for them, are falling back on the facts of ethics. In the beginning, thirty years ago, they were created out of ethics; they were born auspiciously. What a welcome they had! Tennyson and Browning and Ruskin, Westcott and Martineau, the late Lord Shaftes-

bury, and her Majesty the late Queen—these all, and many more, among whom were some of the best men and women of the Victorian Age, were their friends. There never was a cause that enjoyed a better send-off. Everything was in its favour. Magendie and Schiff and Mantegazza had made people sick of experiments on animals. The advocates of the method had not very much to show on its behalf; no bacteriology, save as a far-off vision; no great discoveries lately in physiology or pathology. Thirty years ago, good and true men fought a way for the Act; and there are few now who think the worse of them for it, or grudge them that victory. But, though ethics may be the same always, yet the arguments from them are not. The ethical argument now—we try to find it, and it takes all shapes, and vanishes in a cloud of foul language. That text about the sparrows, which is never quoted in full; that fear about the vivisection of hospital patients; and all that nonsense about moral shrouds, and serpents in the desert, and developed tastes for blood; and Mr. Bernard Shaw, who on May 22nd, 1900, suggests to the National Society that "*the laceration of living flesh quickens the blood of the vivisector as the blood of the hunter, the debauchee, or the beast of prey is undoubtedly quickened in such ways*,"[1] and a week later, before the London Society, dogmatically postulates humaneness as a condition of worthy personal character; and the lady who says, *Oh, Pharisees and hypocrites! Oh, cruel and ruthless egotists!* and the Falstaff's army of the osteopath, and the fruitarian, and the *anti* this, that, and the other, who follow the cause;

[1] Mr. R. B. Cunninghame-Graham's variant on this theme, in the *Daily News*, Aug. 27, 1903, is really too filthy to be put here. Like Mr. Loraine, I dare not produce my brief.

and all these discordant societies, and the begging for money—where, in all this confusion, can we find the ethical argument? Mercy is admirable, but I will wait till mercy and truth are met together. Let us leave the societies to their ethics, and see what they have to say for themselves in the lower realms of science.

I

First, there are the general arguments. That experiments on animals are useless, or of very little use; that they contradict each other; that you cannot argue from animals to men, or from an animal under experiment to a man not under experiment; that the discoveries made by the help of experiments on animals might have been made as well, or better, without that help; that the way to advance medicine and surgery has been, and is, and always will be, not by experiments on animals, but by clinical and *post-mortem* studies. These and the like arguments we may call general; they are the complement of the horrible stories and magic-lantern slides of the itinerant lecturer.

1. The vague statement that these experiments are of little use, may be answered in several ways. It does not come well from those who say that the question is ethical, not utilitarian; who neither know, nor care, nor are agreed, what is the real value of these experiments. "I challenge you," says one, "to show me what good they have done." Another says, "I admit that they may perhaps have done a little good; but so little; they are a bad investment; you would get a better return from other methods of work." Another says, "I don't care whether they have or have

not done good; this is a matter of conscience; we must not do evil that good may come; I grant all, or nearly all, your instances—malaria, and diphtheria, and cerebral localisation, and so forth; but the question is a moral question, and we must not inflict pain on animals, save for their own good." Probably the best answer is, that good has indeed come, and is coming, and so far as we can see will come, out of these experiments; that the instances given are indeed true; that these results were won out of many failures, and contradictions, and fallacies, and harkings-back; and that they have stood the test of time, and will underlie all better results, all surer methods, that shall take their place.

2. The statement that "you cannot argue from animals to man" is not true. Why should it be? Take tubercle, tetanus, or rabies. The tubercle-bacillus is the same thing in a man, a test-tube, or a guinea-pig; the virus of rabies is transmitted from dogs to men; oysters harbour typhoid, fleas carry the plague, diverse mosquitoes carry malaria, yellow fever, filariasis, and dengue. Take the circulation of the blood, the nature and action of the motor centres of the brain, the vaso-motor nerves, the excretory organs, the contractility of muscle, the blood-changes in respiration—where are the differences to support this statement that you cannot argue from animals to men?

3. The twin statements, that all the results got by the help of experiments might have been got some other way, and that clinical study and *post-mortem* study are infinitely more fruitful than experimental study, may be taken together. We are told that anybody could have discovered the circulation by injecting the vessels of a dead body. Well, Malpighi tried to discover the capillaries by this method, and failed. We

are asked to admit that phrenology, long before physiology, discovered the truth about the surface of the brain; *I have been told*, says Mr. Coleridge at an annual meeting of his society, *that the physiologists can now triumphantly map out the human brain. I think the phrenologists have always been able to do that, and whether they or the vivisectors do it best does not much matter.* We are told that the use of thyroid extract could have been discovered right away by mere chemistry and thinking. We hear of a proposal for a bacteriological laboratory on anti-vivisectionist principles, where no inoculations shall be made. This argument, that the whole thing might have been done some other way, must repair its wit, and find better instances. Then comes the incessant appeal: "Stick to clinical work; study diseases at the bedside, in the *post-mortem* room, in the museum, anywhere but in the laboratory. The Hospital taught you to neglect these methods; it made experiments on its patients, it cheated the public, it sheltered malignant cruelty in its most repulsive form under illustrious patronage. Set aside pathology; just sit by your patients long enough; that is the way of discovery."

Or the appeal takes another tone: "Stick to sanitation. If only everybody were healthy, everybody would be well. Diseases are due to dirt, to vice, to overcrowding, to want of common-sense. Abolish all slums, disinfect all mankind, body and soul, make every house clean and wholesome, no bad drainage, or ventilation, or water, or food. Leave your torture-chambers, and open your eyes to the blessed truth that, if everybody were healthy, and everybody were good, everybody would be well." What is the use of talking in this way? Suppose that all the physiologists suddenly rushed into practice, and all the bacteriologists were

turned into medical officers of health. What would be gained? What difference would it make? The physiologists, of course, would merely vivisect their hospital patients; and the bacteriologists would hardly feel the change, for many of them are medical officers of health already, public servants, appointed by the State.

This argument, that practice is fruitful of discoveries, and science is barren of them, reaches its highest absurdity in the National Society's official journal; which praises extravagantly those methods of practice which were not discovered by the help of experiments on animals; praises them without experience, criticism, or understanding. It finds a statement, in the *Medical Annual*, that a year has passed without any great improvement in practice; and at once it lays the blame not on practice but on science. It fights hard against a fact which began in science, though it has been proved a thousand times over in practice. It accuses the bacteriologists now of caring nothing for human suffering, now of rushing after every new method of treatment and flooding the market with drugs. *There is money in the business*—that is the phrase of the *Zoophilist*. But there is money, also, in the anti-vivisection business. *If you can provide for the society's future in your will, may we beg of you to do so? If you agree, pray do it now*, says the London Society: *this is the most alive humane organisation in the world.* But the National Society says, *A grave injury is done to the cause of mercy by the deplorable waste of money spent in perfectly unnecessary offices and salaries. We say that one office would amply suffice for all the work, and that one office would not need half-a-dozen paid secretaries.*

II

Let us leave the general arguments and come to the special arguments. Some of them are concerned with the experiments themselves, some with the men who made them, some with the administration of the Act. These special arguments must be arranged in some sort of order ; but they cross and recross, and are of diverse natures, and any attempt at strict arrangement would fail. That the arrangement may be useful for immediate reference, and may help anybody to answer statements made at debates and lectures, a separate heading has been given to each argument. Those arguments are put first which are concerned with the experiments themselves, or with the men who made them ; afterward come those which are concerned with the administration of the Act.

Harvey

"It is perfectly true," says Mr. Berdoe, "that Harvey again and again, in the plainest terms, declares that his experiments on living animals aided him in his discoveries." I agree here with Mr. Berdoe. Then comes this sentence: *But that is not so important as it appears to be.* Why not? What is gained by this attempt to explain Harvey away? Dr. Bowie mistranslates him ; Dr. Abiathar Wall half-quotes him ; Mr. Adams says that Harvey did not ascribe his discoveries to experiments on animals ; Mr. Berdoe says that he did ; and Mr. Berdoe's society withdraws every pamphlet that involves acceptance of Dr. Bowie's mistranslation. Why should we take, on Harvey's work, any opinion but that of Harvey?

Sir Charles Bell

For the argument from Sir Charles Bell's words, and for the truth about his work, see Part I., Chap. VII.

Cerebral Localisation

Mr. Berdoe says that it is "pure nonsense" to argue from the motor areas of a monkey's brain to those of a man's brain. Why is it nonsense? What is the difference between the movement of a group of muscles in a monkey's arm and the same movement of the same group of muscles in a man's arm? With a very weak current, so weak that it is not diffused beyond the area where it is applied, the surface of a monkey's brain is stimulated at one spot; and forthwith its opposite arm is flexed, or its opposite leg is drawn up, or whatever the movement may be, according to the spot. A man has some disease, acute or chronic, of his brain; and, as the disease advances, twitchings occur in one arm or one leg, little irrational useless movements, or rigidity, or loss of power, according to the case. Is it pure nonsense to believe that the disease has reached a certain spot on the surface of his brain? There is no question here of the mental differences between men and monkeys; no question of consciousness or of will. But Dr. Holländer, who thinks very highly of Gall's system of phrenology, says, *Is the laboratory-man, the experimental physiologist, to teach us the mental functions of the brain from his experiments on frogs, pigeons, rabbits, dogs, cats, and monkeys?* That is the argument; that we must not compare the monkey's motor areas with the man's motor areas, for we cannot find the mind of a man in the brain of a frog.

But, putting aside phrenology, which is a broken reed for anti-vivisection to lean on, what other arguments are urged against the facts of cerebral localisation? First, that the speech-centres were discovered without the help of experiments on animals. That is true; and there, practically, the work of discovery stopped, till experiments on animals were made. Next, that the physiologists have not always been agreed as to the facts of cerebral localisation; that Charcot doubted them, that Goltz criticised Munk, and so on. What is the date of these doubts and criticisms? They are twenty years old. Next, that the surgery of the brain often fails to save life. That is true; and the anti-vivisection societies make frequent use of this fact. But they are unable to suggest any better method. Mr. Berdoe tells us that he cannot remember hearing, in his student days, anything about brain-experiments on animals:—

"Our work was to observe as closely as possible the symptoms and physical signs exhibited by patients in the hospital wards who suffered from any form of nerve or brain disease, and having carefully noted them in our case-books, to avail ourselves, when the patient died, of any opportunity that was offered us in the *post-mortem* of correcting our diagnosis."

That is an exact picture of the state of things thirty years ago; the student taking notes, waiting for the *post-mortem* examination, then correcting his notes there, etc. Every case of brain-tumour in those days died, but many are saved now; and every case of brain-abscess in those days died (one or two were saved by a sort of miracle of surgical audacity); but many are saved now.

Antitoxins and Carbolic Acid

It is said by opponents of experiments on animals, that the active principle, in antitoxin, is not the antitoxin, but the carbolic acid which is added to it. They take this statement from the *Medical Brief;* and we have learned something of the style of that journal. Here is a sentence from the official journal of the National Society:—

"The *Medical Brief* calls antitoxin 'the fraud of the age,' and says: *Would that physicians could all realise the hideous horror of using this nasty stuff as a remedial agent.* It would be nothing less than ghoulishness to inject the matter from an abscess into a child's arm, yet antitoxin is not much better; it is the decomposing fluid from a diseased horse, partially neutralised by carbolic acid."

For a commentary on this sentence, take the following letter from an eminent bacteriologist:—

"As regards diphtheria antitoxin, the addition of an antiseptic is by no means necessary or universal. For fully two years I added none to the serum which I prepared, but contented myself with filtration through a Kieselguhr filter, and bottling under aseptic conditions. At one time Roux used to put a small piece of camphor in each bottle as some sort of safeguard against putrefaction. Nowadays I believe that most makers preserve their sera by adding a trace of trikresol—I am not quite sure of the amount, but it is either .04 per cent. or .004 per cent.!"

But it is probable that the *Zoophilist* will still accept the authority of the *Medical Brief.* Baccelli got good results, in tetanus, from the administration of carbolic acid; therefore, in diphtheria, the good results from

diphtheria-antitoxin are due to the carbolic acid in it. That is the argument. But there is no carbolic acid in it? Oh, then the patient got well of himself, the treatment didn't kill him, it was not diphtheria after all, the disease has altered its type lately, he was well nursed, the back of his throat was painted with something, the doctor got half-a-crown by calling it diphtheria, the bacillus diphtheriæ may be found in healthy mouths, and all bacteriology is *base and blatant materialism*.

The Argument from the Death-rate

There is another argument against diphtheria-antitoxin; we may call it, for brevity, the death-rate argument. It is this. *The doctors say that the antitoxin does save lives; they give us statistics from every part of the world. But, if it saves lives, then the total mortality ought to go down. But the Registrar-General's returns do not go down; indeed, they tend to go up. Therefore diphtheria-antitoxin is useless, or worse than useless.* By this kind of logic, umbrellas are useless. If they were useful, then the more umbrellas there were, the less rain there would be. But the increase in umbrellas coincides with a positive increase of rain. Therefore umbrellas are useless, or worse than useless.

Despite the absurdity of this argument, Mr. Coleridge and Mr. Somerville Wood, the National Society's lecturer, have worked hard with it; Mr. Coleridge in the press, Mr. Wood on the platform. Surely this confusion between the total mortality and the case-mortality of an epidemic disease is a very serious offence. That there may be no doubt of the confusion, let us consider a set of quotations, out of a correspondence published

in September–October 1902, between G. P., whose initials we may take to mean general practitioner, and Mr. Somerville Wood. This correspondence is a good instance of the argument in its usual form :—

G. P.: "The antitoxin treatment of diphtheria has lessened the mortality from that disease by nearly 50 per cent. In the hospitals of the Metropolitan Asylums Board the average case-mortality for the last five years of the pre-antitoxin period, *i.e.* previous to 1895, was 30.6; that for 1895 and the successive four years was 18.1, the successive figures being 22.8, 21.2, 17.7, 15.4, and 13.6, the mortality steadily falling with increased familiarity with the use of the remedy. This has not been the result of a diminished virulence of the disease, as similar experience has been gained all over the world. The figures for Chicago are even more striking, as the averages are 35.0 and 6.79 for the pre- and the post-antitoxin periods respectively."

Mr. WOOD: "Nowadays, almost every sore throat is called diphtheritic, antitoxin is given, and wonderful statistics are formulated to bolster up the latest medical craze. The real test is whether the introduction of antitoxin has lowered the death-rate generally from diphtheria. Here are the Registrar-General's figures: In 1887, the death-rate from diphtheria per million persons in this country was 140. In 1897, after the treatment had been used several years, the death-rate from this disease increased to 246 per million."

G. P.: "Mr. Wood's statistics do not vitiate my argument in the very slightest. His selected figures, using the lowest rate since 1881, merely show that diphtheria as a whole was more prevalent in 1897 than in 1887. He cannot and does not attack the statement that the case-mortality has been lessened where antitoxin has been used, and his test is no test at all."

Mr. WOOD: "Let me give the annual death-rate from diphtheria to a million living persons from 1881 to 1900,

taken from the Registrar-General's returns." (Gives them.)

G. P.: "One last word in answer to Mr. Wood. I repeat that his figures show nothing more than the accepted fact that diphtheria as a whole has been increasing for the last 30 years. This has no bearing at all on the also accepted fact that where antitoxin is used the mortality is lessened, and Mr. Wood has not, in fact, denied this. His confusion of total mortality and case-mortality only shows that he does not understand the elementary principles of statistics."

A few weeks later, at the *Bournbrook and Selly Oak Social Club*, Mr. Wood gives his "thrilling lecture, with lantern views," *Behind the Closed Doors of the Laboratory:* one of his stock lectures. In it, he says:—

"The proof of the pudding was in the eating. In 1881 the death-rate from diphtheria was 127 per million; in 1900 it was 290 per million. He had but to state that the antitoxin treatment was introduced about 1894."

Four days later, at an *overflowingly-attended Citizen Social* at Birkenhead:—

"The proof of the pudding lay in the eating. In 1881 in each million of the population 121 persons died from diphtheria, while in 1900 the mortality from the same disease was 290 persons in each million of the population, and the antitoxin treatment was introduced in 1894."

A few weeks later, at Ipswich, the same thing. This time, he is challenged by letters in the *East Anglian Daily Times*, and again quotes the Registrar-General.

A few weeks later, at the *Hyde Labour Church:* the *Closed Doors of the Laboratory* again:—

"He found from the Registrar-General's returns that the death-rate had gone up in cases in which they were told that wonderful things had been done by experiments

on living animals. If a lower death-rate could be shown, then the vivisectionists might have something to go upon; but they could not show a lower death-rate."

That was in January 1903. In December 1903, Mr. Wood is still using the same argument; this time it is a lecture at Ashton on *Vivisection and the Hospitals:*

"Again and again had they defied the so-called scientific world to put their finger on the Registrar-General's returns, and show them a single instance where the death-rate had been lowered by vivisection, and they had not been able to do it. On the contrary, he found that the death-rate had gone up in the last 20 years, despite the thousands of animals that had been experimented upon. The death-rate in diphtheria was 100 per million more than it was in 1878."

Mr. Wood in the provinces, and Mr. Coleridge in the papers, have used this argument hard. Let us look at it well. It has been refuted again and again. Take a thousand cases of diphtheria from any civilised part of the world, in the days before antitoxin; how many of them died? Take a thousand cases now, treated with antitoxin; how many of them die? Why do Mr. Wood and Mr. Coleridge run away from that easy question? There is nothing unfair in it; they have all the reports before them; they know the facts well. We do not find any evidence that they are willing to acknowledge the truth of those facts. Follow Mr. Somerville Wood, from place to place, with his magic-lantern and his stock of lectures. The lantern-pictures are many of them taken from foreign sources, and some of them are of great age; but they include a portrait of Mr. Coleridge, and some comic slides to be shown at the end of the lecture, rabbits vivisecting a professor, and so forth. Certainly, he works hard; 95 lectures in one year;

we cannot better employ the funds at our disposal than in sending well-informed lecturers to every city in the kingdom to rouse the just indignation of the people. The year after that, 74 lectures; *on two occasions he has spoken when unsupported to over* 1000 *people, and an audience of several hundreds is quite the rule.* Here he is at Windsor, with Bishop Barry in the chair, and he says to them :—

"Unhappily, Pasteur left his microscope and chemicals and took up the vivisectionist's knife. In that he got utterly astray and became nothing more than a mere quack."

Here, with a different audience, at the Mechanics' Lecture Hall, Nottingham, giving his lantern-lecture on *Pasteurism* to a *most respectable audience of working men, their wives, sons, and daughters, and in many cases children.*

"The thesis he set out to elaborate and maintain was that Pasteurism produces hydrophobia rather than cures it; that vivisection under any circumstances is both cruel and immoral; and that with special reference to bacterial toxicology and the treatment by inoculation, the preparation of toxins by the Pasteur methods was the most horrible form of repulsive quackery and hideous cruelty."

Here he is at Birmingham, asking for money, and hinting that, unless all experiments on animals are stopped, *the poor will be the ultimate victims.* Here, at Gloucester, saying that *it is silly to experiment at all,* and that he is not going to take his views as to right and wrong from any man of science, however learned he may be. Here, at Edinburgh, with the *Closed Doors* again, and the picture of the rabbit "roasted alive": three grains of opium, he tells them, would be enough to kill the strongest navvy in Edinburgh, but 16 grains can be administered to a pigeon; and the death-rate

has gone up every year in spite of vivisection. Here, at a drawing-room meeting, asking for money ; here, at a garden party, with a *considerable number of persons ranging themselves on the grass*, and he tells them that they have on their side all that is best in every department of public life ; here, at Blackburn, with the *Closed Doors* again, calling the law *a sham and a farce;* here, at Cheltenham, with Bishop Mitchinson in the chair, still quoting the Registrar-General, and saying that *he does not think the outlook was ever more promising than it is to-day.* All over the kingdom, he and his magic-lantern, year after year, goes Mr. Wood. He is a fluent speaker; he has things in his pocket ; they are brought out, if you contradict him ; or he "challenges" you, or explains you away, or says that you "are not quite playing the game." Let him alone ; to-morrow he will pack up his lantern, and be gone.

Mr. Coleridge, in his use of the death-rate argument, carries it even further than Mr. Wood ; for he applies it over a wider range. "Look at myxœdema," he says ; "the doctors tell us that they can cure it with thyroid extract, and that the use of thyroid extract was discovered by the help of experiments on animals. Very good. Myxœdema is due to some fault in the thyroid gland. Very good. But here are the Registrar-General's returns of the annual death-rate for all diseases of that gland. See, the death-rate has gone up, steadily, during the last 20 years." Was there ever such an argument? It is only of late years that myxœdema has been generally recognised. Till it was recognised, it was not diagnosed ; till it was diagnosed, it was not returned as a cause of death. Again, there are many other diseases of the thyroid gland, including various forms of malignant disease. It is cancer of the

thyroid gland that decides the death-rate. The number of deaths from myxœdema, especially since the discovery of thyroid extract, must be small indeed. Moreover, apart from Mr. Coleridge's fallacy of argument, it is impossible to see how he can really doubt the efficacy of the thyroid treatment, both in myxœdema and in sporadic cretinism.

Again, "Look at the diseases of the circulation," he says. "The doctors say that digitalis and nitrite of amyl act on the heart; and that the action of these drugs was discovered by the help of experiments on animals. Very good. The heart is concerned with the circulation. Very good. But here are the Registrar-General's returns of the annual death-rate for all diseases of the circulation. See how it has gone up, from 1371 per million persons in 1881 to 1709 in 1900. Therefore, either these two drugs are never used, or they are useless, or the Registrar-General's returns are false." It is impossible to understand how Mr. Coleridge could bring himself to write thus. Digitalis has a certain effect on the heart-beat; nitrite of amyl diminishes arterial tension. The Registrar-General's returns for all diseases of the circulation include every sort and kind of organic disease of the valves of the heart; include also pericarditis, aneurism, senile gangrene, embolism, phlebitis, varicose veins, and 35,499 deaths from "other and undefined diseases of heart or circulatory system."

Rabies

For rabies, Mr. Berdoe praises the "Buisson Bath Treatment for the Prevention and Cure of Hydrophobia." The virtues of this treatment are proclaimed

by the Chairman of the Canine Defence League, F. E. Pirkis, Esq., R.N., of Nutfield, Surrey, and it is founded, we are told, *on the simple common-sense principle that if poison is injected into a person's veins the best thing to do is to get it out as quickly as possible.* This sentence, and the reference to Mr. Pirkis for further particulars, and the fact that there is, or was, a Buisson Bath at the "National Anti-vivisection Hospital," bring us to the question, What is the value of the evidence in favour of this treatment?

Mr. Berdoe, in his Catechism of Vivisection (1903), gives this evidence at considerable length. *The treatment,* he says, *is simplicity itself. It is merely the use of the vapour bath, which causes a free action of the skin to be set up, this draws the blood to the surface of the body, and so relieves the congestion of the internal organs.* Let us consider this sentence. (1.) Suppose that X—— were bitten by a mad dog, say on March 1st, and on March 8th he took a course of Buisson Baths, for safety's sake. There would be no congestion, at that period, of his internal organs; what would be the good of drawing the blood to the surface of his body? Mr. Pirkis says that there would be poison in his veins; it would be a very subtle poison. How can Mr. Pirkis tell that it is all in his veins and none of it elsewhere? Again, X—— would be feeling perfectly well. How would a vapour-bath get this poison out of his veins? It could not do it by relieving the congestion of his internal organs, for they would not be congested. How would it do it? And how would Mr. Pirkis know when it had done it? (2.) Suppose that X—— were bitten by a mad dog, and, in due time, were seized by hydrophobia. Has Mr. Pirkis ever seen a case of that disease —ever seen a case of hydrophobia? Are they going to

tie X—— down, or steam him under chloroform, or what? And how many baths would he want? But there are cases; there is evidence; a "mass of cures in Asia." Let us look at them; and let us divide them into cases of prevention and cases of cure. Let us take, first, the cases of cure.

There are five of these. Five, and no more. One is Dr. Buisson; cured by one bath, while he was trying to commit suicide; nothing said about the dog. One is a case at Kischineff, near Odessa, 18 years ago; no evidence is given that the dog was rabid. One is a case at Arlington, New Jersey, 18 years ago; no evidence is given that the dog was rabid. One is the case of Pauline Kiehl; no date; no reference to say where the case is published; no account of her symptoms. And one is a case at the Jaffna Hospital, Ceylon; no date; and nothing said about the dog. Of these five cases, three were a boy, a lad, and a little girl; but their ages are not given. Five cases in 20 years; they hail from all parts of the world, France, Russia, the United States, Ceylon, and France again; three of them happened 18 years ago, or more. And, we may be certain, not one of them is genuine. Spurious hydrophobia, the simulation of the disease out of sheer terror of it, as in Dr. Buisson's case, is well known.

Now we come to the cases of prevention. Over 80 of them, we are told; but seven are especially noted. Four in 1895, under the care of Dr. Ganguli of Dinajpur; two in 1896, under the care of Dr. Dass of Narainganj; and one in 1896, Mr. Kotwal of Bassein. Of this "mass of cures in Asia," we all know what would have been said if Pasteur had been in charge of them; that the dogs were not rabid, that the bites were not infected, that the wonder is that the poor deluded victims were not added to Pasteur's hecatomb.

Next, what does Mr. Berdoe say of the division of all patients at the Pasteur Institute into classes A, B, and C? Does he admit that a dog is proved to have been rabid, if a minute portion of its nervous tissue, taken from it after death, and put into a rabbit, causes the rabbit to have paralytic rabies? No; there are still two things left for him to say:—

1. He says, on the authority of the *Veterinary Record* of ten years ago, that *the death of a rabbit with cerebral symptoms is not a positive indication of death from rabies.*

2. He says that Vulpian discovered that healthy human saliva was poisonous to rabbits, and that it contained a micro-organism which Pasteur had also found in the saliva of a rabid patient. What does this statement prove or disprove? It is twenty-five years old; but Mr. Somerville Wood, not long ago, used it at a debating society with great fervour.

Also Mr. Berdoe quotes the late M. Peter, Dr. Lutaud's forerunner; quotes an *obiter dictum* of Professor Billroth, but without any date; tells us that Pasteur himself, in a letter, referring to one particular case, declared cauterisation to be a sufficient preventive, but does not tell us the date of the letter, or the facts of the case; and quotes a death-rate, but stops at 1890. Of course, any method of treatment, if you ransack its records over a sufficient number of years, will show, now and again, failures or disasters. Take, for instance, those methods of light-treatment, which Mr. Berdoe praises so highly. They have had many failures, and one or two disasters. If they had been discovered by the help of experiments on animals, we might have had a pamphlet from the National Society, *The Roentgen "Cure": its list of Victims.*

Certificate A and Certificate B

Frequent use has been made of some words spoken by the Home Secretary in Parliament, on July 24th, 1899. He was asked whether he would state what rules were laid down with regard to the granting or signing of certificates dispensing with the use of anæsthetics in experiments on animals; and whether there was any limit to the number of such certificates which one person might sign, or to the number of experiments upon different animals which might be performed by the person holding one such certificate. There can be no doubt as to the meaning of these questions. Certificate A, which is granted only for inoculation experiments or similar proceedings, and never for any serious cutting operation, dispenses wholly with anæsthetics. Certificate B, which is granted for any kind of operation *plus* observation of the animal after operation, dispenses partly with anæsthetics; that is to say, the operation is done under an anæsthetic, and the subsequent observation of the animal, which is counted as part of the experiment, is made without an anæsthetic. The questions come to this: When the Home Office grants Certificate A, or Certificate B, what precautions does it take against any abuse of these certificates, and what restrictions does it impose on them?

The Home Secretary answered:

"It is the practice of the Home Office, in addition to the fact that all certificates expire on December 31st of the year in which they are granted, to limit the number, and this is always done in the case of serious experiments in which the use of anæsthetics is wholly or partly dispensed with."

The *Times* says that the Home Secretary said "serious experiments." Mr. Coleridge says that *Hansard* says that the Home Secretary said "serious operations." We need not doubt that Mr. Coleridge is right; but we may doubt whether Hansard underlines the word *wholly*, as Mr. Coleridge does. Anyhow, it does not matter now whether the Home Secretary, seven years ago, said *experiments* or *operations*. His meaning is clear enough; that, in all serious procedures, whether they be under Certificate A or under Certificate B, a limit is put to the number of experiments. Which is the plain truth, as everybody knows who is concerned in the administration of the Act; and the limit may be very strict indeed. After this statement by the Home Secretary in 1899, we still find Dr. Abiathar Wall, the Hon. Treasurer of the London Anti-vivisection Society, saying in 1900 that a *vivisector has only to say that he has a theory whereby he hopes to discover a cure for, say, neuralgia of the little finger, and the Home Secretary promptly arms him with a license to torture as diabolically as he pleases and as many animals as he deems fit.* And the National Society has made constant use of this phrase about "serious experiments"; declaring that the Home Secretary himself has said that animals are tortured under the Act. Here are three statements to that effect, made by the National Society's Parliamentary Secretary, by its Lecturer, and by its Hon. Secretary:—

1. (Annual Meeting, Queen's Hall, May 1900.)—"If you are still unconvinced—if any one is not thoroughly satisfied that there is ample cause for the anti-vivisectionist movement to-day—it is only necessary for me to refer you to the words of the Home Secretary, as spoken in Parliament, in the year 1898.[1] He said:

[1] This should be 1899.

'There are serious operations which are performed, during which the use of anæsthetics is wholly or partially dispensed with.' Could there be any more sweeping indictment than that? Is there any need for me to attempt to convince you that the lower animals are vivisected painfully, after the words officially spoken by the Home Secretary in the House of Commons?"

2. "If you want any further proof I will quote from Hansard, July 24th, 1899, when the then Home Secretary stated in the House of Commons that serious experiments take place under the law of England, in which the use of anæsthetics is wholly or partially dispensed with. Now, I affirm that serious experiments in which anæsthetics are wholly or partially dispensed with mean torture pure and simple."

3. (Annual Meeting, St. James's Hall, May 1901.)—"If this were not enough, the late Home Secretary has told us the facts. I have Hansard here. On July 24th, 1899, the late Home Secretary in his place in Parliament, and in his official capacity as Home Secretary, told us that 'serious experiments, in which the use of anæsthetics have been wholly or partially dispensed with,' do take place in English laboratories. We know, therefore, that torture does take place."

Each of the three speakers uses this phrase as a final and irresistible argument. *If you are still unconvinced. If you want any further proof. If this were not enough* —they all of them play the Home Secretary, as a sure card: at Queen's Hall, at St. James's Hall, they produce him as though it were indeed unanswerable. Since they are willing to go back to July, let us take them back to May. This phrase about "serious experiments" was spoken on July 24th, 1899. On May 9th of that year, a question was put and answered in the House. It was put by the same gentleman who put the question in July; it was answered by the same Home Secretary;

and it was practically the same question. The Home Secretary, in his answer to it, said:—

"The sole use of this Certificate (B) is to authorise the keeping alive of the animal, after the influence of the anæsthetic has passed off, for the purpose of observation and study. I should certainly not allow any certificate involving dissections or painful operations without the fresh use of anæsthetics."

Here, in May 1899, we have this emphatic statement, that Certificate B is *not* allowed for "serious operations without anæsthetics." Why did the National Society stop at July? If it had only gone a few weeks further back, a surprise was in store for it. But at July it stuck; thus it was still able to say all sorts of things about "legalised torture." So late as May 6th, 1902, at the great annual meeting at St. James's Hall, the Rev. Reginald Talbot said:—

"Certificate B makes it necessary that the operator should produce complete anæsthesia during the initial operation, but (please mark this) after the initial operation is over, after the animal has returned to the state of semi or complete consciousness, there is then allowed by this certificate a period of observation upon a semi-sensible or completely sensible animal. The animal is opened, is disembowelled, and in that condition his vital organs can be probed and stimulated. Now that is something more than pain; it deserves something more than the name of even severe and prolonged pain. Surely this comes within the tract and region of what we may call agony."

As for Certificate A, the inoculations-certificate, which is used for inoculations only, and therefore is granted for nine experiments out of every ten, he said:—

"There is a Certificate A, which, if it were granted, and when it is granted—and pray you mark my words,

for I know what I am speaking about, and I want you to know too—would allow major operations to be performed upon animals, cats, dogs, or any other animals, without the use of any anæsthetic at all. I know quite well that that certificate has not been applied for, or has not been granted this last year, or, so far as I know, in any previous year, but I say this," &c.

It is impossible to understand these words. Certificate A is never granted for major operations. It is never granted (save in conjunction with another certificate) for any sort or kind of experiment on a cat or a dog, or a horse, or an ass, or a mule. It is more in use than all the other certificates put together; it covers nine experiments out of every ten. We shall try in vain to guess how this mistake arose in the speaker's mind. But, at the great annual meeting of the chief of all the anti-vivisection societies, it is strange indeed that nobody seems to have corrected him. This description of a certificate which does not exist—*I know what I am speaking about*, he says, *and I want you to know too*—was applauded by an audience that filled the whole hall. Nobody on the platform put him right. And, in the next number of its official journal, the National Society reported every word of his speech, and said that he had *analysed the Act and its administration in a striking and powerful manner.*

Curare

"Curare," says Mr. Berdoe, "paralyses the peripheral ends of motor nerves, even when given in very minute doses." That is to say, it prevents all voluntary motion. Then comes this frank admission, "Large doses paralyse the vagus nerve and the ends of sensory

nerves." That is to say, it can be pushed, under artificial respiration, till it paralyses sensation. With small doses, the ends of the motor nerves lose touch with the voluntary muscles. With large doses, under artificial respiration, the ends of the sensory nerves lose touch with the brain. Let us agree with Mr. Berdoe that curare does act in this way; that it does not heighten sensation, and has no effect, save in very large doses, on sensation, and then abolishes sensation. Only, of course, to procure this anæsthetic effect, the animal may have to be subjected to artificial respiration.

(The evidence as to the action of curare on the sensory nerves rests not on the case of accidental poisoning recorded by Mr. White, though that case does point that way, but on Schiff's experiments on the local exclusion of the poison from one leg of the frog by ligature of an artery.)

This, surely, is a true definition of curare, that it is a painless poison, which in small doses prevents the transmission of motor impulses; and, in large doses, which may necessitate the use of artificial respiration, prevents the transmission of sensory impulses. Mr. Berdoe can hardly refuse to accept this definition; indeed, it is his own. And, certainly, he would be a bold man who said that a small dose of curare has any effect on sensation; or that the exact strength of any one specimen of curare is standardised as a supply of antitoxin is standardised.

Now we have a perfect right to take a practical view of curare. At the present time, and in our own country, how is it used? The Act forbids its use as an anæsthetic. What evidence does Mr. Berdoe bring that it is so used?

1. He quotes Professor Rutherford's experiments. These were made at least 16 or 17 years ago.

2. He quotes Dr. Porter's paper, "On the Results of Ligation of the Coronary Arteries." (*Journal of Physiology*, vol. xv. 1894, p. 121.) Dr. Porter speaks of four experiments made under morphia *plus* curare. These experiments were made at Berlin, 14 years ago, by the Professor of Physiology at Harvard, U.S.A.

3. He refers to Professor Stewart's papers, in the same volume of the *Journal of Physiology*. The one experiment which he quotes at some length was made at Strasburg, 14 years ago or more.

But we want to know what is done now and here under the Act, not what was done at Berlin or Strasburg 14 or more years ago. Still, the experiments by Professor Stewart have been in constant use, among the opponents of all experiments on animals. In May 1900, at the great annual meeting of the National Society, at Queen's Hall, Dr. Reinhardt said:—

"I will pass on to prove to you, by a few conclusive evidences, for which I can give you chapter and verse, that torture is inflicted on animals by British vivisectors to-day. Now, if you buy the 15th volume of the *Journal of Physiology*, and look at page 86, you will find there," etc.

To prove that animals are tortured in England to-day, he quotes one experiment made at Strasburg ever so long ago. And, in 1901, Mr. Coleridge wrote, in the *Morning Leader*, saying: *It is with curare, which paralyses motion and leaves sensation intact, that all the most shocking vivisections are performed.* And, the same year, Mr. Stephen Smith, a "Medical Patron" of the London Society, wrote: *I state emphatically that when curare is used, proper anæsthesia is out of the question.* . . .

Curare is used daily throughout England. Mention of an anæsthetic in a report is no guarantee that the animal was anæsthetised.

I cannot find, in all the anti-vivisection literature which I have read, any shadow of evidence that any experiment of any sort or kind has been made in this country, on any sort or kind of animal, under curare alone, for the last sixteen or seventeen years. I believe that I might go further back than that. But surely that is far enough.

Certainly, so long as any curare is used (not as an anæsthetic, but in conjunction with an anæsthetic) in any experiments on animals in this country, the societies will not trouble to inquire how much of it is used. I wrote, therefore, to the Professors of Physiology at Edinburgh, Cambridge, and Oxford, and asked them to tell me how much curare was used in their laboratories throughout 1903, and what anæsthetics were given with it. Some opponents of experiments on animals seem to think that curare is used very often. One of them says that it is "used daily throughout England." So I wrote to these Professors at our Universities, and they kindly sent the following answers:—

1. "Your question *re* curare is easily answered. We did *no* experiments with it during the past year. Indeed, I have given it up almost entirely for years, chiefly because it is very difficult to get a preparation which—I suppose from impurities—does not seriously affect the heart. There might still be occasions during which it is necessary to use it—if, *e.g.* the *least* muscular movement would vitiate the results of an experiment. But I find it possible in nearly all cases to get such absolute quiescence with morphia or chloral (besides ether and chloroform) that to all intents and purposes I have long given up the

use of curare. Of course, if I had occasion to use it, an anæsthetic would be administered at the same time."

2. "I have asked those who worked in the physiological laboratories in 1903 to give me a return of the number of experiments done and of the number in which curare was used. Including my own experiments, I find that 160 in all were made under the License and Certificates B, EE, C. Curare was given in four cases; in two of these the A.C.E. mixture was the anæsthetic, in the other two ether."

3. At the third laboratory, during 1903, curare was given to seven frogs deprived of their brains before it was given, and to one rabbit under ether.

That was the whole use of curare, during a whole year, in three great Universities: at one, seven inanimate frogs, and one rabbit under ether; at another, four animals, under A.C.E. or ether; at another, nothing.

Incomplete Anæsthesia

It sometimes happens, at an operation, that the patient moves. Mostly, this movement is at the moment of the first incision through the skin; but it may be at some later period during the operation. He does not remember, after the operation, that he moved, or that he felt anything. That is incomplete anæsthesia, or light anæsthesia. The corneal reflex may be abolished, and still the patient may move.

Seven years ago some experiments were made in this country by an American surgeon. In the published account of them, it was said that one of the animals was, at one time, under incomplete anæsthesia, and that, in the case of another animal, the anæsthesia was at one time overlooked. This latter phrase meant not

that the anæsthetic had been left off, but that it had been given in excess, so that the blood-pressure suddenly fell. The character of the experiments, and the occurrence of these two phrases about the anæsthesia, roused some criticism, and the Home Office instituted an inquiry into the matter. "That inquiry," it said, October 11th, 1899, "resulted in showing no evidence whatever that the animals experimented on by Dr. Crile felt pain. On the contrary, all the evidence shows they did not." The Act does not go into questions of corneal reflex, and unconscious muscular movements, and all the undefinable shades between incomplete anæsthesia and complete anæsthesia and profound anæsthesia. "The only substantial question," says the Home Office, "is whether or no the animal has been during the operation under the influence of an anæsthetic of sufficient power to prevent it feeling pain. This is the requirement of the law." We cannot refuse to call morphia and chloral anæsthetics, for there are deaths every year from an overdose of them. And we cannot admit that an animal under an anæsthetic, because it makes a movement, is in pain or is conscious ; for we know that a patient under operation may move yet feel nothing. Every hospital surgeon, and every anæsthetist, who has seen a whole legion of patients go under chloroform or ether and come out of it, and everybody who has been under these anæsthetics, they all know that incomplete anæsthesia is not "sham anæsthesia," and that movements, even purposive movements, may occur without consciousness, without pain, alike in men and in animals.

One Animal and One Experiment

When the Home Office allows a licensee to make a certain number of experiments, it means that he may experiment on that number of animals and no more. The Home Office, having heard what the experiments are to be, where they are to be made, on what kind of animals, and for what purpose, and having taken advice about them, allows him to make a fixed number, and adds any restrictions that it likes, *e.g.* that he must send in a preliminary report when he has made half that number. And one thing is certain, that one experiment = one animal, and that 10 experiments = 10 animals, and no more. Everybody knows that, who knows anything at all about the administration of the Act.

Now take a false statement, which has been made again and again during many years, that one experiment = any number of animals, and observe how it spread.

1. In the House of Commons, on March 12th, 1897, Mr. MacNeill asked whether any record were kept of the number of animals used in experiments during 1895, and said that 200 or 300 animals are sometimes used in a single experiment, and that 80 or 90 is a common number. The Home Secretary answered: "The honourable member is under an entire misapprehension. The number of animals used does not exceed the number of experiments given in the return."

2. A year later, May 18th, 1898, at the Annual Meeting of the National Society, Mr. MacNeill said again: "Any one casually reading that report (the Inspector's report to Government) would imagine that each experiment was on the body of a single animal. It is nothing of the kind. An experiment is a series of

investigations in some particular branch, and sometimes 20, 30, or 40 animals are sacrificed in the one experiment." The National Society published this speech in its official journal.

3. A few weeks later, an anonymous letter in the *Bradford Observer* said, "Any one casually reading the report would imagine that each experiment was on the body of a single animal. It is nothing of the kind. An experiment is a series of investigations in some particular branch, and sometimes 20, 30, or 40 animals are sacrificed in the one experiment."

4. On August 1st, 1898, the National Society published this letter in its official journal, under the heading, "Our Cause in the Press."

5. On October 21st, 1902, a letter in a provincial paper said that "one experiment" means "not one animal, but a series of operations on many animals."

6. In January 1903, the National Society admitted that its action in 1898 (see 4) was "unfortunate."

7. On June 25, 1903, in Parliament, Mr. MacNeill again said that "an experiment" did not mean one operation, but a series of researches, "often performed by persons who had no more skill than the children who broke up a watch."

8. About this time, the same false statement was made by an Anti-vivisection Society at Manchester.

9. A little later, it was made by the National Canine Defence League, in these words, "Each experiment may include any number of dogs. There is no limit fixed by law." On January 11th, 1904, in the *Times*, the leaflet containing this and other "grossly false and misleading statements" was vehemently denounced by the National Society.

It would be hard to find a better instance of the

spreading of a false report. An experiment? Oh, it is any number of animals—20 of them, 30 of them; 200, 300 of them; hecatombs, and triple hecatombs; any young doctor can get leave to cut them up.

Certificates E and EE

For all inoculations and similar proceedings, Certificate A is necessary. For all experiments where the animal is allowed to recover from the anæsthetic, Certificate B is necessary. But these certificates do not extend to the dog, the cat, the horse, the mule, or the ass. The three latter animals are also scheduled under Certificate F; the dog and the cat under Certificates E and EE. That is to say, to inoculate a dog, *e.g.* for the study of the preventive treatment against distemper, it is necessary to hold a License, *plus* Certificate A, *plus* Certificate E; to operate on a dog, and let him recover, it is necessary to hold a License, *plus* Certificate B, *plus* Certificate EE.

And it is certain that the Home Office does enforce and emphasise here the spirit of the Act; and that it does guard and restrict and tie up Certificate EE with its own hands.

Now let us take an instance, which shows in a very unfavourable light the methods of the National Canine Defence League. Three years ago, certain experiments were made on dogs, for the purpose of finding the best way of resuscitating persons apparently drowned. The Home Secretary was asked whether he knew that certain of these experiments were to be made without anæsthetics; and he answered, "In view of the great importance of the subject in connection with

the saving of human life, and of the strong recommendations received in support of the experiments, I have not felt justified in disallowing the certificates."

A great outcry was raised against these experiments by the National Anti-vivisection Society and the Canine Defence League. The National Society, in its official journal, August 1903, said that it was now proved, "that in England to-day experiments are performed without anæsthetics which involve inconceivable agony to dogs, and this with the deliberate permission of the Home Secretary." Mr. Coleridge made a public appeal to all humane societies, to go down with all their strength into Kent, on that not far distant day when the Home Seeretary would have to face his constituents, and turn him out of Parliament. The Canine Defence League sent two memorials to the Home Office, circulated a petition, and issued leaflets, entitled *A National Scandal*, *Scientific Torture*, *A Peep behind the Scenes*, and so forth. We must consider one of these leaflets at some length; but first let us see what is the truth about these experiments. They were made by the Professor of Physiology at Edinburgh; and he has kindly written to me about them. *In every experiment, except two, the animal was, throughout the whole experiment, under complete anæsthesia with chloroform or ether. In two cases, and in two only, a small preliminary operation, under anæsthesia, having been performed, the animal was allowed to recover from the anæsthetic, or almost to recover from it, and was then and there submerged and drowned, at once and completely, to death; no attempt at resuscitation was made; it became unconscious in a little more than a minute.*

In the face of these facts, what is to be said of

the outcry raised by the Canine Defence League? They presented two memorials to the Home Secretary: they got up a monster petition with thousands of signatures; and they issued the following leaflet:—

SIGN THE

NATION'S PETITION

TO PARLIAMENT AGAINST THE

DISSECTION OF LIVE DOGS

In Medical Laboratories

1. Dogs, on account of their docility and obedience to the word of command, are the animals chiefly selected for torture.

2. Thousands of dogs are tortured yearly by licensed experimenters.

3. The total number of experiments performed in 1902 was 14,906, 12,776 of which were without anæsthetics.

4. The Home Secretary stated in Parliament on July 22nd, 1903, that neither the starving of animals to death nor the forced over-feeding of animals were included in these returns.

5. Nor does the number 14,906 give the number of dogs used, for each experiment may include any number of dogs—there is no limit fixed by law.

6. The Home Secretary stated in Parliament on May 11th, 1903, that at one laboratory alone in London 232 dogs were used for vivisectional experiments last year.

7. There are now laboratories scattered over the whole of the United Kingdom.

8. The Home Secretary stated in Parliament on 10th July 1903, that one dog may be used *again* and *again* for vivisectional experiment or demonstration—and this without anæsthetics.

Think of the condition of the poor dog between each living-dissection.

Has not the time come for the nation to rise as one man and say—

"This shall not be"?

It is no wonder that even the National Anti-vivisection Society, in a letter to the *Times*, December 11th, 1903, denounced this leaflet. The wonder is, that Mr. Pirkis, R.N., the chairman of the Canine League, tried to defend it. *This deplorable leaflet*, said the National Society: *It contains a series of grossly false and misleading statements.* Let us take it paragraph by paragraph. The first two paragraphs are grossly false. The third suppresses the truth. The fourth is grossly false; the Home Secretary said that neither the starving of animals to death nor the forced over-feeding of animals was included among the experiments *authorised or performed.* Paragraph five is grossly false. So is paragraph six: not one word was said about any experiments, either by the Home Secretary or by anybody else. The entire number of all dogs and cats together, under Certificates A, B, E, and EE, throughout the whole kingdom, that year, was 344. Paragraph eight is grossly false.

For want of space, it is impossible to consider all the special arguments of the anti-vivisection societies. Of course, among these special arguments, there are a few which have something in them. How could they all of them be utterly false? They go back over thirty years; they are drawn from all parts of the world. This incessant rummaging of medical books and journals, British and foreign; and all this everlasting espionage;

the whole elaborate system of a sort of secret service—these methods, year in year out, are bound to find, now and again, a fault somewhere. But I do say, having read and re-read a vast quantity of the publications of these societies, that they are, taken as a whole, a standing disgrace to the cause; that they are tainted through and through with brutal language, imbecile jokes, and innumerable falsehoods; that they have neither the honesty, nor the common decency, which should justify them. Still, here it is that the money goes. There is *money in the business;* there is *milk in the cocoa-nut;* and *twopence more, and up goes the donkey.* These are the phrases used, by the National Anti-vivisection Society, of the bacteriologists, and the men who are working at cancer. But these societies, that spend thousands every year, what have they got to show for it all? They have, with much else of the same kind, the *Zoophilist.* Truly, a fine result; a high-class official journal, the *recognised organ of the anti-vivisection movement in England.*

Take, for a final instance, one or two of the things said about anæsthetics. On June 12th, 1897, in the *Echo*, Mr. Berdoe said that certain experiments, involving severe operations, had been made on dogs under morphia and curare. He based this assertion on the account of the experiments in the *Journal of Physiology.* On June 18th, Mr. Weir, in the House of Commons, called attention to this assertion; and the Home Secretary promised to inquire into the matter. On July 18th, Mr. Weir asked whether this inquiry had been made; and the Home Secretary answered:—

"Yes, I have made full inquiry into the allegations contained in the letter and statement which the honourable

member forwarded to me, and find that they are absolutely baseless. The experiments referred to were performed on animals under full chloroform anæsthesia; the morphia, to which alone allusion was made in the published account of the experiments, being used in addition. Curare was used, but not as an anæsthetic."

It is simple enough. The gentlemen who made the experiments did not know that the National Society buys and ransacks the *Journal of Physiology;* or did not care. But the National Society called this answer a "Fruitless Official Denial"; and Mr. Coleridge sent an "explanatory letter" to the London daily papers, accusing all the experimenters of "amending their published record so as to make it fit in with the Government report." In 1899, the National Society published that sentence, which has already been quoted, about the *Nine Circles,* and the "whiff of chloroform possibly administered." In 1900, it said, "The chloroformists of the physiological laboratories are doubtless common porters, with no technical knowledge of their work." In 1901, it said, "Our readers will remember that Mr. Coleridge has had more than one battle with the Home Office on the question of complete and incomplete anæsthesia. We need hardly say that the victory on each occasion rested with our Honorary Secretary." And again, "By many turns of the anti-vivisection screw we have at last extracted (from the Home Office) the admission that pain is not unknown in the laboratories." In 1902, it said, "The blessed word anæsthesia warns off the profane anti-vivisectionist who would rob the altars of science of their victims." Take later instances. In 1903, we find Mr. Wood saying that *we may be sure the narcosis becomes profound when the inspectors knock at the door of the laboratory;* Dr.

Brand, saying that *in all experiments, other than inoculations, it is probable that only a whiff of chloroform is given, to satisfy the experimenter's conscience, and to enable him to make humane statements to the public;* and Mr. Berdoe, saying that *vivisectors, where they use anything except curare, employ sham anæsthetics.*

Beside such statements as these, there is the argument from the very rare action of morphia as a stimulant (see *British Medical Journal*, January 14th, 1899); but this argument is not in question. The real argument is, that a man who makes experiments on animals is likely enough to tell lies about them. As Mr. Berdoe says, of a very explicit statement about anæsthetics, made by the late Professor Roy, *It is and must be absolutely untrue.* Read again that sentence about the "whiff of chloroform." The phrase is thirty years old; but, like Sir William Fergusson's evidence in 1875, it is still in use. Or take that one phrase—*where they use anything but curare.* It affords, in six words, a perfect instance of the anti-vivisectionist at his worst.

IV. "Our Cause in Parliament"

Under this heading the official journal of the National Society reports questions asked in Parliament, and the answers given to them. This aspect of the work of the anti-vivisection societies, and the part taken by them in elections, and their plans to amend or abolish the Act, must be noted here.

In one year, the National Society spent £888, 13s. 2d. on "purely electoral work." That is a very large sum, when we think of *the grave injury done to the cause of mercy by the deplorable waste of money spent in perfectly*

unnecessary offices and salaries. The Society's journal tells us something of this electoral work:—

1899.—"The Parliamentary League has again been successful in its work at bye-elections. At —— the two candidates were approached, and both gave more or less satisfactory answers. Sir ——'s reply was thought to be the more satisfactory one, and consequently our supporters gave him their votes. As our readers are aware, he was returned." (In a later number, the *Zoophilist* hints that "further pressure" may be applied to this gentleman in Parliament.)

1900.—"The efforts of the Society will not be confined to forwarding the interests of any one candidate or any one party. As soon as the names of candidates were announced, Mr. Coleridge issued to all of them a circular letter demanding their views on the vivisection question. The numerous replies which have already arrived, and are still arriving, afford results more gratifying than we for a moment anticipated, and show clearly that we are now recognised throughout Great Britain to be a power that cannot be ignored. . . . Volunteer workers are also being despatched from headquarters to various places. Readers who have votes or who will help in any way are invited to communicate immediately to the head office, when information about the views of their candidates will be at once sent to them."

The London Society also, like the National Society, desires to have a representative in Parliament; and this desire is stated in emphatic words in one of its reports. The general tone of that report has already been noted. It loves big black headlines, No Surrender, The Awakening Churches, A Truculent Science, The Sinews of War, The Appeal to the People. They had better ensure the return of that opponent of vaccination who says that you can bring any member of Parliament to your knees.

And, of course, these societies follow the successful candidates on their subsequent careers. "In Parliament," says the London Society, "the Society's work is carried on as occasion permits. Members of Parliament are written to or are personally seen at the House of Commons. Questions are drafted for them to submit to the Home Secretary, and one or more officers of the Society are in constant attendance at the House of Commons when the question of vivisection is likely to be raised." And the National Society says, "In order to stimulate attention (to Mr. Coleridge's Bill) our lecturer has been assiduous in his attendance in the lobby of the House during the present session, and by personal interviews has been able to arouse a good deal of interest in it on both sides of the House." It is evident that "Our Cause in Parliament" is urged with diligence, and is not left to stand or fall according to the unsolicited conscience of what the London Society calls the *average lay member*. Take, for example, the system of drafting questions to be put to the Home Secretary. It may or may not take off the edge of sincerity; anyhow, the question should be drafted with great care. On February 26th, 1900, a question was asked as to certain observations which were alleged to have been made on living animals, but in fact had been made on their organs removed after death. The National Society said of this mistake:—

"We wish our readers to know that the question was not prompted by any communication from our Society, and we think it unfortunate that members of Parliament should be asked to put questions in the House by persons who do not realise that questions based on inaccurate premises can do nothing but harm to our cause. It is

hard that the whole anti-vivisection movement should suffer through the carelessness and indolence of those who will neither be at the pains to avoid inaccuracy by their own study and investigation, nor by consulting the National Society's officers."

These careless, indolent, inaccurate persons, who think so lightly of the National Society's officers, and draft a question so silly that the whole cause is damaged, bring us back to the point whence we started: the want of unity between the societies, the frequent jarring of one with another. We have still to see something of the dealings of the National Society with Government. It is at its best, doubtless, in the formal letters from Mr. Coleridge to the Home Office; but these, after all, are his own work, and the Society cannot take the credit of them. *Per contra*, we may credit to the Society, and not to Mr. Coleridge, certain threats to Ministers in 1898:—

. . . "Should we be so unfortunate as to be left by you without such an open assurance, we shall feel it our duty to employ the strength and resources of this Society in an endeavour to prevent your return to Parliament at the next election. We know of a large and increasing number of your constituents who are ready, in the unfortunate event of your being unable to reassure them as to your attitude in the matter of endowing torture, to place humanity above party politics."

. . . "This Society will feel it to be its duty to use every means in its power to prevent your return to Parliament at the next election."

. . . "We beg leave to inform you that at the next election the forces of this Society will be used with the utmost vigour to prevent your return to Parliament. We know of many, and shall no doubt soon secure more of your constituents, pledged to place humanity above party

and vote against you on the next occasion that you present yourself."

What are we to think of these three letters? The resources of the Society, given with some vague hope of keeping animals out of pain, are to be used for keeping Ministers out of Parliament. Note the bullying tone of the letters. It is the same thing, two years later, at the General Election, with the heckling of candidates: *We are now recognised throughout Great Britain to be a power that cannot be ignored.* A Society that bullies Ministers of State, what will it not do to the average lay member?

V. A Historical Parallel

It is a long way, from the plain duty to take care of animals, to the arguments and general behaviour of these societies. Of course, we have seen them here from the most unfavourable point of view. From that point of view, apart from any more favourable aspect, they have their parallel in history. The two instances are, in some ways, very unlike: but the parallelism is worthy of note. The historical instance is more than fifty years old: we have what was said, in 1851, against his worst opponents, by a man who had an unpopular cause to defend. Newman, in 1851, gave a set of lectures on *The Present Position of Catholics in England:* and his sayings, some of them, seem apt to our present subject. Take the following examples. Only, here and there, a word is altered, or a phrase left out, that all offence may be avoided:—

. . . "We should have cause to congratulate ourselves, though we were able to proceed no further than to persuade our opponents to argue out one point before

going on to another. It would be much even to get them to give up what they could not defend, and to promise that they would not return to it. It would be much to succeed in hindering them from making a great deal of an objection till it is refuted, and then suddenly considering it so small that it is not worth withdrawing. It would be much to hinder them from eluding a defeat on one point by digressing upon three or four others, and then presently running back to the first, and then to and fro, to second, third, and fourth, and treating each in turn as if quite a fresh subject on which not a word had yet been said."

. . . "No evidence against us is too little: no infliction too great. Statement without proof, though inadmissible in every other case, is all fair when we are concerned. An opponent is at liberty to bring a charge against us, and challenge us to refute, not any proof he brings, for he brings none, but his simple assumption or assertion. And perhaps we accept his challenge, and then we find we have to deal with matters so vague or so minute, so general or so particular, that we are at our wits' end to know how to grapple with them."

. . . "For myself, I never should have been surprised, if, in the course of the last nine months of persecution, some scandal in this or that part of our cause had been brought to light and circulated through the country to our great prejudice. No such calamity has occurred: but oh! what would not our enemies have paid for only one real and live sin to mock us withal. Their fierce and unblushing effort to fix such charges where they were impossible, shows how many eyes were fastened on us all over the country, and how deep and fervent was the aspiration that some among us might turn out to be a brute or a villain."

. . . "We are dressed up like a scarecrow to gratify, on a large scale, the passions of curiosity, fright, and hatred. Something or other men must fear, men must loathe, men must suspect, even if it be to turn away their minds from their own inward miseries. . . . A

calumny against us first appeared in 1836, it still thrives and flourishes in 1851. I have made inquiries, and I am told I may safely say that in the course of the fifteen years that it has lasted, from 200,000 to 250,000 copies have been put into circulation in America and England. A vast number of copies has been sold at a cheap rate, and given away by persons who ought to have known that it was a mere fiction. I hear rumours concerning some of the distributors, which, from the respect which I wish to entertain towards their names, I do not know how to credit."

. . . "The perpetual talk against us does not become truer because it is incessant; but it continually deepens the impression, in the minds of those who hear it, that we are impostors. There is no increase of logical cogency; a lie is a lie just as much the tenth time it is told as the first; or rather more, it is ten lies instead of one; but it gains in rhetorical influence. . . . Thus the meetings and preachings which are ever going on against us on all sides, though they may have no argumentative force whatever, are still immense factories for the creation of prejudice."

. . . "The Prejudiced Man takes it for granted that we, who differ from him, are universally impostors, tyrants, hypocrites, cowards, and slaves. If he meets with any story against us, on any or no authority, which does but fall in with this notion of us, he eagerly catches at it. Authority goes for nothing; likelihood, as he considers it, does instead of testimony; what he is now told is just what he expected. Perhaps it is a random report, put into circulation merely because it had a chance of succeeding, or thrown like a straw to the wind; perhaps it is a mere publisher's speculation, who thinks that a narrative of horrors will pay well for the printing: it matters not, he is equally convinced of its truth: he knows all about it beforehand; it is just what he always has said; it is the old tale over again a hundred times. Accordingly he buys it by the thousand, and sends it about with all speed in every direction, to

his circle of friends and acquaintance, to the newspapers, to the great speakers at public meetings. . . . Next comes an absolute, explicit, total denial or refutation of the precious calumny, whatever it may be, on unimpeachable authority. The Prejudiced Man simply discredits this denial, and puts it aside, not receiving any impression from it at all, or paying it the slightest attention. This, if he can: if he cannot, if it is urged upon him by some friend, or brought up against him by some opponent, he draws himself up, looks sternly at the objector, and then says the very same thing as before, only with a louder voice and more confident manner. He becomes more intensely and enthusiastically positive, by way of making up for the interruption, of braving the confutation, and of showing the world that nothing whatever in the universe will ever make him think one hair-breadth more favourably than he does think, than he ever has thought, and than his family ever thought before him. About our state of mind, our views of things, our ends and objects, our doctrines, our defence of them, he absolutely refuses to be enlightened. . . . The most overwhelming refutations of the calumnies brought against us do us no good at all. We were tempted, perhaps, to say to ourselves, 'What *will* they have to say in answer to this? Now at last the falsehood is put down for ever, it will never show its face again.' Vain hope! Such is the virtue of prejudice—it is ever reproductive; future story-tellers and wonder-mongers, as yet unknown to fame, are below the horizon, and will unfold their tale of horror, each in his day, in long succession."

. . . "Perhaps it is wrong to compare sin with sin, but I declare to you, the more I think of it, the more intimately does this Prejudice seem to me to corrupt the soul, even beyond those sins which are commonly called more deadly. And why? because it argues so astonishing a want of mere natural charity or love of our kind. They can be considerate in all matters of this life, friendly in social intercourse, charitable to the poor and outcast, merciful

towards criminals, nay, kind towards the inferior creation, towards their cows, and horses, and swine; yet, as regards us, who bear the same form, speak the same tongue, breathe the same air, and walk the same streets, ruthless, relentless, believing ill of us, and wishing to believe it. They are tenacious of what they believe, they are impatient of being argued with, they are angry at being contradicted, they are disappointed when a point is cleared up; they had rather that *we* should be guilty than *they* mistaken; they have no wish at all we should not be unprincipled rogues and bloodthirsty demons. They are kinder even to their dogs and their cats than to us. Is it not true? can it be denied? is it not portentous? does it not argue an incompleteness or hiatus in the very structure of their moral nature? has not something, in their case, dropped out of the list of natural qualities proper to man?"

These sentences, many of them, might be used now to describe Anti-vivisection at its lowest level. It might keep a higher level: but we have seen that the literature, arguments, and general methods of the Anti-vivisection Societies fail to do that. The Parliamentary interviewer, the itinerant lecturer, and the letter-writer, are not, after all, of much help to any cause: and surely it is time, after all this waste of huge sums of money, that a Royal Commission should inquire, not only into experiments on animals, but also into Anti-vivisection.

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